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NEW CONSTRUCTIONS IN GRAPHICAL STATICS.

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IV.

ARCH RIB WITH END JOINTS.

Let the curve a of the arch to be treated have a span of six times the rise, as represented in Fig. 6, and having divided the span into twelve equal parts, make the ordinates of the type bd twice the ordinates ab.

Let a uniform load having a depth xy cover the two-thirds of the span at the left, and a uniform load having a depth $xy' = \frac{1}{2}xy$ cover the one-third of the span at the right. Assume any pole distance, as of one-third of the span, and lay off $b_1w_1 = xy$ = one-half of the load supposed to be concentrated at the center; $w_1w_2 = 2xy$ = the load concentrated above b_1, etc. Similarly at the left make $b_4'w_1' = xy$ = one-half the load above b; $w_1'w_2' = 2xy$ = the load above b_1'; $w_2'w_3' = xy + xy' = \frac{3}{2}xy$ = the load above b_2'; $w_3'w_4' = \frac{1}{2}xy$ = the load above b_3', etc.

From this force polygon draw the equilibrium polygon c, just as in Figs. 2 and 3.

Now the closing line of the equilibrium polygon for a straight girder with ends free to turn, must evidently pass so that the end moments vanish. Hence c_6c_6' is the closing line of the polygon c, and b_6b_6' is the closing line of the polygon d, drawn according to the same law. The remaining condition by which to determine the bending moments is:

$$\Sigma(M_d - M_c)y = 0 \quad \therefore \quad \Sigma(M_d y) = \Sigma(M_c y)$$

which is the equation expressing the condition that the span is invariable, the summation being extended from end to end of the arch.

This summation is effected first as in Figs. 2 and 3, by laying off as loads quantities proportional to the applied moments concentrated at the points of division of the arch, and so find the second equilibrium polygon, or deflection polygon of two upright girders, bent by these moments.

Let us take one-fourth of the ordinates bd as these loads, *i. e.* $bm = \frac{1}{4}$ of $\frac{1}{2}bd$; $mm_1 = \frac{1}{4}b_1d_1$, etc.: also bn, nn_1, etc., equal to similar fractions of the ordinates of the curve c. Using d as the pole for this load, we obtain the total deflection bf_6 on the left, and the same on the right (not drawn) due to the bending moments M_d.

Similarly g_6g_6' is the total deflection right and left due to the moments M_c.

Now the equation of condition requires that $\frac{1}{2}g_6g_6' = bf_6$. That this may occur, the ordinates of the polygon c must be elongated in the ratio of these

deflections. To effect this, make $ai = \frac{1}{2} g_0 g_6'$ and $aj = bf_6$, and on the horizontals through i and j at a convenient distance draw the vertical $i_0 j_0$; then the lines ai_0 and aj_0 will effect the required elongation, as previously explained. To obtain the center ordinate be, for example, make $ai' = bh$ $\therefore$ $aj' = be$. To find the new pole o, draw bv parallel to $c_6 c_6'$ and vo horizontal, as before explained.

If ai_0 cuts the load line at t_1 and the horizontal through t_1 cuts aj_0 at t_2, then the vertical through t_2 is the new position of the load line and tt_2 is the new horizontal thrust.

Now using o as the pole of the load line $u_1 u_1'$ etc., through t_2 draw the equilibrium polygon starting from e. It must pass through b_6 and b_6', which tests the accuracy of the construction.

The construction may now be completed just as in Fig. 3, by doubling the pole distance, and finding the tangential thrust along the arch and the normal shear directly across the arch in the segments into which it is divided. The maximum thrust and tangential stress is obtained when the line load covers the entire span.

To compute the effect of changes of temperature and other causes of like nature in producing thrust, shear, bending moment etc., let us put the equation of deflections in the following form:

$$mD_y \cdot \frac{EI}{mn^2n'} = \Sigma\left(\frac{M}{nn'} \cdot \frac{y}{n}\right) \quad . \quad (D)$$

This equation may perhaps put in more intelligible form the processes used in Figs. 4 and 5, and is the equation which should be used as the basis for the discussion of temperature strains in the arch. In equation (D) n is the number by which the rise of the arch must be divided to reduce it to bd, *i.e.*, it is the scale of the vertical ordinates of the type bd, in Fig. 6, so that if bd was on the same scale as the arch itself, n would be unity. Again n' is the scale of force, *i.e.*, the number of tons to the inch; and m is a number introduced for convenience so that any assumed pole distance p may be used for the pole distance of the second equilibrium polygon. In Fig. 6, $p = bd$.

We find m from the equation,

$$p = \frac{EI}{mn^2n'} \quad \therefore \; m = \frac{EI}{pn^2n'}$$

from which m may be computed, for EI is a certain known number of foot tons when the cross-section of the rib is given, p is a number of inches assumed in the drawing, n and n' are also assumed. Now D_y is the number of inches by which the span is increased or decreased by the change of temperature, and mD_y is at once laid off on the drawing.

The quantities in equation (D) are so related to each other, that the left-hand member is the product of the pole distance and ordinate of the second equilibrium polygon, while the right-hand member is the bending moment produced by the loading $M \div nn'$, which loading is proportional to M. The curve f was constructed with this loading, and only needs to have its loads and ordinates elongated in the ratio of bf_6 to $\frac{1}{2} mD_y$ to determine the values of $M \div nn'$ at the various points of division of the arch. One-half of the quantities is used, because we need to use but one-half the arch in this computation. Two lines drawn, as in Figs. 4 and 5, effect the required elongation.

The foregoing discussion is on the implied assumption that the horizontal thrust caused by variation of temperature is applied in the closing line bb_6 of the arch, which is so evident from previous discussions as to require no proof here.

The quantity determined by the foregoing process is $M \div nn' = q$ say, a certain number of inches. Then $M = nn'q$, and $H = My \div n'q \div \frac{y}{n}$, in which $\frac{y}{n}$ is the length of the ordinate in inches on the drawing at which M is applied.

The determination of the shearing and tangential stress induced by H is found by using H as the diameter of a circle, in which are inscribed triangles, whose sides are respectively parallel and perpendicular to the segments of the arch, precisely as was done in Figs. 4 and 5.

The whole discussion of the arch with end joints may be applied to an unsymmetrical arch with end joints. In that case, it would be necessary to draw a curve f'' at the right as well as f at the left, and the two would be unlike, as g and g' are.

This, however, would afford no difficulty either in determining the stresses due to the loads, or to the variations of temperature.

When the live load extends over two-thirds of the span, as in the Fig., the maximum bending moment is nearly in the middle of that live load, and is very approximately the largest which can be induced by a live load of this intensity, while the greatest moment of opposite sign is found near the middle of the unloaded third of the span.

If the curve of the arch were a parabola instead of the segment of a circle, these statements would be exact and not approximate, as may be proved analytically. This matter will be further treated hereafter.

ARCH RIB WITH THREE JOINTS.

Let the joints be at the center and ends of the arch, as seen in Fig. 7. Let the loading and shape of the arch be the same as that used in Fig. 6. Now since the bending moment must vanish at each of the joints, the true equilibrium curve must pass through each of the joints; *i. e.*, every ordinate of the polygon c must be elongated in the ratio of db to bh. To effect this, make $di=bh$, and at a convenient distance on the horizontals through b and i draw the vertical $i_0 b_0$. Then the ratio lines di_0 and db_0 will enable us to elongate as required, or to find the new pole distance ti, diminished in the same ratio, by drawing the horizontal ti through i_0. The new pole o is found in the same manner as in Fig. 6.

Now with the new pole o and the new load line through t, we can draw the polygon e starting at d. It must then pass through b_0 and b_0' which tests the accuracy of the construction.

The maximum thrust, and tangential stress is attained when the live load covers the entire span.

Variations in length due to changes of temperature induce no bending moments in this arch, but there may be slight alteration in the thrust, etc., produced by the slight rising or falling of the crown due to the elongation or shortening of the arch. This is so small a displacement that it is of no importance to compute the stresses due to it. We have for the same reason, in the previous and subsequent constructions, omitted to compute the stresses arising from the displacement which the arch undergoes at various points by reason of its being bent. It would be quite possible to give a complete investigation of these stresses by analogous methods.

The construction above given is applicable to any arch with three joints. The arch need not be symmetrical, and the three joints can be situated at any points of the arch as well as at the points chosen above.

THE ARCH RIB WITH ONE END JOINT.

Let the arch be represented by Fig. 8, in which the load, etc., is the same as in Fig. 6.

The closing line must pass through the joint, for at this joint the bending moment vanishes.

A second condition which must be fulfilled is that the total deflection of a straight girder having one end joint below the tangent at the fixed end vanishes, for the position of the joint is fixed. This is expressed by the equation

$$\Sigma(Mx)=0,$$

in which the summation is extended from end to end.

This condition will enable us to draw the closing line of the polygon c, and also that of d. The problem may be thus stated:—In what direction shall a closing line such as $c_6 h'$ be drawn from c_6 so that the moment of the negative triangular area $c_6 c_6' h'$ about c_6 shall be equal to the movement of the positive parabolic area $c_6 b c_6'$

To solve this problem, first find the center of gravity of the parabolic area by taking it in parts. The parabolic area $c_6 \angle c_2'$ is a segment of a single parabola whose area is $\frac{2}{3} b_6 b_2' \times c_6 c_2 = \frac{1}{2} h_1 \times b_6 b_6'$, when h_1 + the height of an equivalent triangle having the span for its base $\therefore h_1 = \frac{3}{4} c_2 c_2'$.

Lay off $l_6 b_6 = c_6 c_2$, and draw $l_6 b_2'$ $\therefore b_2' l_6 = h'$. Lay off $c_2' p_1 = h_1$ as proportional to the weight of the parabolic area. Again, $c_2' p$ is proportional to the weight of the triangle $c_6 c_2' c_2'$. The parabolic area $c_2' c_6' = \frac{2}{3} c_6' c_6' \times b_2' b_6' = \frac{1}{2} h_2 \times b_6 b_6'$, as before, $\therefore h_2 = \frac{3}{4} c_6' c_6'$, which may be found as h_1 was before.

Let $h_1 = pp_2$, then on taking any pole, as c_2, of this weight line, we draw $qq_1 \parallel c_2 c_2'$, since the left parabolic area has its

center of gravity in the vertical through q_1, and the triangular area in that through q, we draw $qq_1' \parallel c_2p$, to the vertical through q_1', which contains the center of gravity of the right parabolic area. Then $q_1q_2 \parallel c_2p_1$ and $q_1'q_2 \parallel c_2p_2$ give q_2 in the vertical through the center of gravity of the total positive area. The negative area, since it is triangular, has its center of gravity in the vertical through c_2'.

Now if the total positive bending moment be considered to be concentrated at its center of gravity and to act on a straight girder it will assume the shape rq_2r_1 of this second equilibrium polygon, and if a negative moment must be applied such that the deflection vanish, the remainder of the girder must be r_1r_2, a prolongation of rr_1. Now draw $c_2p_0 \parallel rr_1$ and we have $p_2p_3=c_2'h'$ the height of the triangle of negative area. Hence c_2h' is the closing line, fulfilling the required conditions.

Again to draw the closing line b_2k' according to the same law, we know that the center of gravity of the polygonal area d is in the center vertical. To find the height p_1p', of an equivalent triangle having a base equal to the span, we may obtain an approximate result, as in Fig. 2, by taking one twelfth of the sum of the ordinates of the type bd, but it is much better to obtain an exact result by applying Simpson's rule which is simplified by the vanishing of the end ordinates. The rule is found to reduce in this case to the following:—The required height is one eighteenth of the sum of the ordinates with even subscripts plus one ninth of the sum of the rest.

Now this positive moment concentrated in the center vertical and a negative moment such as to cause no total deflection in a straight girder, will give as a second equilibrium polygon $rq_2'r_1'r_2'$; and if $cp_3' \parallel rr_1'$, then $p'p_3'=b_2'k'$ is the height of the triangular negative area, and the closing line is b_2k'.

Now the remaining condition is that the span is invariable, which is expressed by the equation

$$\Sigma(M_d - M_c)y = 0, \text{ or } \Sigma(M_d y) = \Sigma(M_c y).$$

Let us construct the deflection curve due to the moments M_d in a manner similar to that employed in Fig. 2. We lay off quantities dm_0, m_0m_1, etc., equal to one-fourth of the corresponding ordinates of the curve d, and dn_0, n_0n_1, etc., one-fourth of the ordinates of the curve c. We use one-fourth or any other fraction or multiple of both which may be convenient. By using b for a pole we obtain the deflection curves f and f' for the moments proportional to M_d, and the curves g and g' for those proportional to M_c.

Now, Prop. IV. requires that the ordinates of the polygon c should be increased so that gg' shall become equal to ff'. Make $di=gg'$ and $dj=ff'$ and draw as before the ratio lines di_0 and dj_0, then the vertical through t is the new position of the load line.

Find the new length of bh which is ke, and with the new pole o, draw the polygon e starting at e. It must pass through b_2. The new pole o is found thus: draw $bo \parallel hh'$, then v divides the weight line into two parts, which are the vertical resistances of the abutments. From v_1 draw $v_1o \parallel kk'$, then the closing line of the polygon e has the direction kk', a single joint at any point of an unsymmetrical arch can be treated in a similar manner.

A thrust produced by temperature strains will be applied along the closing line kk', and the bending moments induced will be proportional to the ordinates of the polygon d from this closing line. The variation of span must be computed not for the horizontal span, but for the projections of it on the closing line kk'. This construction will be like that previously employed. Another effect will be caused in a line perpendicular to kk'. The variation of span for this construction, is the projection of the total horizontal variation on a line perpendicular to kk', and the bending moments induced by this force applied at b_2, and perpendicular to the closing line, will be proportional to the horizontal distances of the points of division from b_2. As these constructions are readily made, and the shearing and tangential stresses determined from them, it is not thought necessary to give them in detail.

ARCH RIB WITH TWO JOINTS.

Let us take the two joints, one at the center and one at one end as represented in Fig. 9. Let the loading, etc., be as in Fig. 6.

The closing line evidently passes through the two joints, as at them the bending moment vanishes.

The remaining condition to be fulfilled is that the deflection of the right half of the arch in the direction of this line, shall be the same as that of the left half.

Let us then suppose that the straight girder $b_6' p'$ perpendicular to the closing line, is fixed at b_6' and bent first by the moments M_d giving us the deflection curve $b_6' f'$ when b_6' is taken as the pole, and the loads of the type *mm* are one-quarter of the corresponding ordinates of the polygon *d;* and secondly, by the moments M_c giving us the deflection curve $b_6' g'$ when drawn with the same pole, and the loads of the type *nn* also one-quarter of the corresponding ordinates of the polygon *c*. It should be noticed that the points at which these moments are supposed to be concentrated in the girder $b_6' p'$, are on the parallels to *kk'* through the points d_1, d_2, etc.

Similarly let ff_3 and $f_3 f_6$ be the deflection curves of the straight girder $d_3 p$ (using d_3 as the pole distance), under the applied moments.

We have used now a pole distance differing from that used in the right half of the arch. These pole distances must have the same ratio that the quantity EI has for the two parts of arch. If EI is the same in both parts of the arch the pole distance must be used to obtain the deflection curves in both sides of the middle. In the same manner the curves gg_3 and $g_3 g_6$ are found. Now must the moments M_c causing the total deflection $p' g' - gg_6 = \frac{1}{2} ai$ be elongated so that they shall cause a total deflection $pf' - ff_6 = \frac{1}{2} aj$. The ratio lines ai_0, aj_0' will enable us to find the new position t_2 of the load line to effect this.

To find *o* the new pole, draw through v_2, which divides the load line into parts which are the vertical resistances of the piers, draw $v_2 o \parallel b_6 k$.

Then draw the polygon *e* as in Fig. 7, starting from *d*. It must pass through b_6. We can find also whether Ke_6' has the required ratio to hc_6' by the aid of the ratio lines, which will further test the accuracy of the work.

Any unsymmetrical arch with joints situated differently from the case considered *ed* can be treated by a like method.

The temperature strains should be treated like those in Fig. 8, which are caused by a thrust along the closing line. Those at right angles to this line vanish as the joints allow motion in this direction.

The shearing and tangential stresses can be found as in Fig. 3. Arches with more than three hinge joints are in unstable equilibrium, and can only be used in an inverted position as suspension bridges. These will be treated subsequently. If the joints, however, possess some stiffness so that they are no longer qinge joints, but are block work joints or analogous to such joints we may still construct arches which are stable within certain limits although the number of joints is indefinitely increased. Such are stone or brick arches. These will also be treated subsequently.

A NEW SYSTEM OF SINKING AT THE CANNOCK AND HUNTINGTON COLLIERY.

From the "London Mining Journal."

THOSE of the public interested in mining enterprises have been aware for some time that a new mode of sinking shafts is about to be tried in England, but the nature of the process to be adopted has not been generally understood. That to which we refer is known as the Kind-Chaudron mode of sinking, being so named from the two eminent engineers who have invented and perfected the process referred to. By this method of sinking a shaft may be sunk through water-bearing strata, and lined throughout with the most perfect iron casing, the whole of the operation being carried on from the surface. At first sight it would appear that such a mode of sinking would be an impossibility,

but the fact that some thirty or forty shafts have been successfully sunk on the Continent on this plan places its feasibility beyond a doubt.

The Cannock and Huntington Colliery Company, under a lease from Lord Hatherton, some time since acquired mining rights over about 1,000 acres at Huntington, in the neighborhood of Cannock, and it having been ascertained by boring conducted by the Diamond Rock-Boring Company that known coal measures lay beneath the land in question, it only remained to be determined how the coal should be reached. The boring, which had been carried down to a depth of 218 yards, disclosed the existence of pebble beds and conglomerate at a depth of from fifty to eighty yards, which were not only charged with water at the rate of 250 gallons per minute, but which freely discharged it through the bore-hole, the water rising to a height above the surface of about ten feet in any pipe placed to receive it. Any shaft, therefore, sunk in that ground would have been, in fact, an artesian well, and as the quantity of water which would be discharged through it was in no way ascertainable, the question of sinking was one for most anxious consideration by the directors of the company. They might possibly, by the erection of very powerful engines and pumps, have drained any shaft being sunk during the progress of the works and while the necessary iron lining or tubbing was placed in it, but the circumstances of the case were so exceptional that they felt justified in adopting any process, however novel, by which the enormous expense of sinking in such strata could be avoided.

Messrs. North & Son, of Dudley, the engineers of the company, had introduced to the notice of the directors the Kind-Chaudron method of sinking, and had pointed out fully its peculiar advantages. A visit by the directors to Belgium and France, where pits in progress of excavation were inspected, confirmed the favorable impression which they had formed of this method of sinking, and, after careful consideration, they determined to take the responsibility of adopting the Kind-Chaudron process at Huntington. An arrangement was forthwith made with M. Chaudron to undertake the sinking of two shafts for the company to a depth of about 140 yards each. Special machinery was ordered, foundry and pit-sheds were erected, and what was not long since farming land turned into the busy scene of which we will endeavor to give a slight sketch.

On approaching the colliery the first thing that will strike the visitor is the very high and novel-shaped wooden shed which stands over pit No. 1 (at the other pit No. 2 is being erected). A large foundry shed and a very fine chimney-shaft will also attract attention. The chief interest at present, however, centers in the operations in the pit-shed. To enter this the visitor will have to mount up steps a height of some fifteen or twenty feet above the surface of the ground. Passing through the door at the top he will find himself in what, for want of a better subject for comparison, we may describe as a very lofty barn with a boarded floor. Having arrived here, he will perhaps ask where the shaft is being sunk, for nothing is to be seen of it. In the middle of the floor, however, there is a circular hole of about one foot in diameter, and, if the boring is going on, through this will pass an iron rod, which is suspended by a gigantic screw and chain to the end of an oak beam some ten feet above the floor. A capstan bar through the rod, manned by four laborers, completes all the boring apparatus that is at first sight visible. The end of the beam from time to time slowly rises for about two feet, and suddenly drops again, letting the rod and all that is suspended to it underneath the floor fall with it, the men at the capstan bar walking slowly round as the boring rod rises and falls.

While this monotonous process is going on, the machinery which works the beam and that which rises the tools can be examined. The beam, which projects into the shed from a building joining on to it, and which we may call an annexe, is of oak, and is about twenty-five feet long; its further end is attached to and is drawn down by a piston working in a large cylinder fixed below the beam, and to this steam is admitted at intervals. The beam, therefore, pulled down by the piston at one end and by the weight of the boring apparatus at the other, oscillates under the control of the engineer.

Having examined the beam, we may pass further on into the annexe, and inspect the steam-engine which supplies the power necessary to raise and lower the boring apparatus. The engine is a powerful one, working a train of cog-wheels, the last carrying a drum round which an enormous rope is wound. This rope, it will be seen, passes up a covered way to the pulley at the top of the pit-shed, where it hangs high up overhead ready for use. The time at length arrives for lowering the dredge or basket, and if the visitor will return to the shed he will see this operation. At a signal from the chief borer the motion of the beam is stopped, two baulks of timber are slid up on each side of the boring-rod, and an iron fork or key is laid on them, which just admits through it the passage of the smaller part of the rod, but not of the shoulder of any screw-joint in it; the beam is slightly lowered, and the whole weight of the underground apparatus is borne by the timber we have just spoken of. The chain and screw with the heavy iron swivel at the end of them are detached from the rod of the boring apparatus, and the connection between them and the oscillating beam is severed. The rope now descends from the top of the pit-shed, and being hooked on to the end of the beam lifts it gently up; the men are ready at their posts, and a roller having been slipped under the other end the whole mass of oak to its iron attachments, weighing some five or six tons, is quietly slid back and laid to rest out of the way for a time as gently as if it were a baby being turned over in its cradle. The rope now descends again, is attached to the boring-rods, and in a few seconds the first length is raised above the ground through the hole in the floor we first mentioned; the iron key is again brought into use, while the upper rod is detached and put on one side. The rod in the drill is at length reached, and when this is made fast to the rope the men rapidly remove the floor boards in the middle of the shed, and the observer finds himself standing at the edge of the pit mouth.

On looking down it will be seen that underneath is a circular shaft of some twenty feet in diameter, full of water. The surface of the water is now, perhaps, not more than fifteen feet below the floor of the shed, but when the lower water-bearing strata are reached will, probably, rise some feet higher. The engine now moves again, and the drill is brought into daylight. As soon as it appears the floor boards are replaced, and this part of the apparatus can be approached and examined. When we call the boring tool a drill we do so for want of a better expression, and not that the word properly describes the implement in question. The one now being used at Huntington, which excavates a shaft between six and seven feet in diameter, is the smallest of those that will be used, and yet its weight is about seven tons. It consists of a heavy wrought-iron beam, on the under side of which are several very formidable teeth. Above stands a vertical iron shaft, which may be about fifteen feet long, and on each end of it are attached heavy beams of timber that serve to guide the machine in the hole it has to form in the earth.

The drill is soon swung beneath a carriage, which runs on rails high up in the shed, and is then passed to one side. As rapidly the large dredge is brought over the pit from the other side of the shed, and attached to the rope. This dredge is, in fact, a gigantic iron bucket, made so as to be capable of being easily turned over that its contents may be emptied out of the large window or opening formed in one side of the shed for the purpose. The bottom of the dredge, however, is hinged, and opens upwards in two parts.

The engine is again started, the dredge disappears from sight, the floor is closed, rod after rod is attached and lowered, and when the last rod is fastened the beam is again brought back to its place, and the dredging apparatus moved up and down a few times, being turned round at the same time by the capstan bar. The beam is again detached, and run on one side, the dredge is raised, and when slung underneath its carriage is pushed forward to the window, which is now opened, and by means of a rope and winch gently turned over, discharging its contents of water and mud, broken stones and sand, into a shoot constructed outside the shed. The mud that comes up is, to a great extent, sufficiently liquid to run away down the ditches and

over the adjacent grounds, and but little spoil at present has accumulated near to the pit mouth.

We have now described the work as far as it has yet been carried on. The larger drill has yet to be used; this will excavate the pit to the full size of about sixteen feet diameter, but the smaller drill always precedes the larger one, and thus a small shaft is always sunk in advance of the full-sized one, the advantage of this being that the earth loosened by the action of the larger drill always falls into the smaller shaft, by which it can be easily removed by the dredge.

There is no doubt that with continued practice the laborers now employed will work with even greater precision than they do at present in handling the very heavy machines whose motions they have to direct, but with the short discipline they have had it will certainly strike every visitor to the colliery that they work more like artillerymen at heavy gun drill than colliers—every tool seems ready to hand, every man knows his place, the work goes on silently and rapidly, the men working no doubt all the better from being in a dry, well-swept, and well-arranged shed, rather than on a pit bank, where dirt, wet, and cold exhaust their energies and impede their progress. The manufacture of the tubbing, which will be carried on near to the pit mouth, and the mode of sinking it when the boring is completed, must form the subject of another notice. At present we have only described a process which can be seen in operation, and which from its completeness and novelty is certainly one of the most remarkable in South Staffordshire.

CENTRIFUGAL FORCE *v.* UNBALANCED FORCE.

By ROBT. D. NAPIER.

From "Engineering."

At the meeting of the British Association in Glasgow, in September last, Professor Tait gave a lecture on "Force," in which he spoke of "centrifugal force" and "accelerating force" as being "absurdities." Some time afterwards another eminent scientist, writing in a scientific journal, stated that centrifugal force had for years been given up in books of science. If the latter statement were true, then such books for instance, as the late Dr. Rankine's mechanical and engineering works are not books of science, as they define and make use of the term without any hint about the possible phantom nature of the force implied by it; and if Professor Tait is correct, then Sir Isaac Newton held views on this subject which were not only erroneous, but positively absurd; this, to say the least of it, is what most persons would be surprised to learn. It will very generally, I think, be admitted that Newton understood the science of dynamics about as well, and was about as unlikely to use incorrect or inappropriate phraseology with regard to it as any one since his time; and I think on examination, we shall find that "centrifugal force" is neither "an absurdity," nor an erroneous notion, nor an inappropriate phrase.

The general statement that "centrifugal force" has been given up in books of science is not true, but it appears to have been given up in *some* books of science. That being so, and the doctrine that centrifugal force is an imaginary quantity, having to some extent been accepted by thinkers on the subject, it becomes worth while to examine the arguments or evidences on both sides of the question.

In advocating the existence of centrifugal force, one may be placed between the horns of a dilemma of very formidable appearance, and yet one which probably never occurred to the mind of Sir Isaac Newton, or if it did he probably saw his way out of the difficulty so easily that he did not think it worth while to notice it, since he appears to have adopted the idea and the name of centrifugal force without giving any hint about the possible fallacy of the one or the inappropriateness of the other.

The dilemma referred to may be put

in the following form: If there is such a force as centrifugal force, which is equal and opposite to centripetal force when a body is moving in a circle, then the direction of the motion of a body may be altered, while all the forces affecting it balance one another; for, if the body is moving at a uniform rate there can be no forces acting on it, otherwise than at right angles to the direction of motion, and these by supposition balance each other. Again, if variation of the direction of motion of a body may take place while all the forces affecting it are balanced, so also may change of velocity under similar conditions; for change of direction is in reality change of velocity at right angles to the actual direction of motion at any instant. Again if it has to be admitted that *in some cases* the rate or direction of the motion of a body may be altered, while all the forces affecting it are balanced, we cannot stop short of the conclusion that this is always the case, and that in fact the forces affecting any body are always balanced whether its motion is uniform or variable.

The dilemma then resolves itself into this; that either there is no such force as centrifugal force or else there is no such thing in nature as an unbalanced force; one or the other must be given up. At first sight this seems to be almost conclusive against the existence of centrifugal force, but I nevertheless contend that the anti-centrifugalists have cut away the horn of the dilemma that should have been left standing, and are now revolving round the wrong one like flies round a candle, so absorbed by its attractive power that they fail to recognize the counteracting centrifugal force.

On the one hand we must deny the existence of a force of which the effects are as manifest and palpable as those of any other force; or, on the other hand, we must conclude that whether a body is moving at a uniform or variable rate the forces by which it is affected always balance one another.

Before beginning to discuss the main argument against the existence of centrifugal force, I wish to dispose of a so-called argument which to some minds appears to be a very powerful one, though it is based on a complete confusion of the ideas of force and motion.

The argument is that there cannot be any force properly called centrifugal, for if the centripetal force suddenly ceases to act, as for instance by the breaking of a cord which was causing a body to move in a circular path, the body does not fly off radially, but in a tangent to the curve. From the report I have seen of the lecture by Professor Tait, I cannot say that he used this argument in direct terms, but I think part of it cannot bear any other meaning; however that may be, I have seen the argument used by at least one first-class and well-known mathematician and by numerous other persons of smaller calibre. If it had not been for this, I should not have felt justified in taking time to refute so absurd an argument.

In order to show its absurdity it is only necessary to carry it out to its legitimate conclusion thus: the body does not fly off radially, *therefore* there was no outward radial force at the time of the breaking of the cord; the body does fly off tangentially *therefore* there was a tangential force existing; but if the body was moving at a uniform rate there could be no tangential force, for if there had been it must have been either increasing or decreasing the velocity. That the body flies off in a tangent is conclusive evidence that that was its direction of motion at the time the centripetal force ceased to act, but it does not furnish the shadow of evidence as to the forces that were acting the instant before it did so.

To produce velocity both time and force are required, and assuming that centrifugal force exists at all, it is the resistance arising from a body being forced out of a straight line, just as centripetal force is the force pulling it out of a straight line, and the instant the one ceases so does the other, when the body at once moves in a straight line, for the element of time is wanting to allow of radial velocity being produced. The crucial question is, whether an unbalanced force is a possibility. If that question can be answered definitely one way or the other, it must settle the whole matter.

In statics, we know that every force must be opposed by an equal and opposite force, or by forces whose resultant is equivalent to that, at least I presume

this will not be disputed. That is to say if A presses against B with a given force, then B will press against A with an equal force. In dynamics, we *assume* that a force may act on a body without being opposed by an equal force, and that the result is motion imparted to the body. I shall endeavor to show by reasoning and examples, that when motion is being produced there is a resisting force which limits the rate at which volocity may be produced by a given accelerating force, and that these two forces are equal. If it be objected that there would be no effect if the resisting force were equal to what may be called the acting force, my reply is that this view of the case overlooks the circumstance that the resisting force arises entirely from the fact that the result is actually taking place.

A very clear illustration of my meaning may be seen in the case of a ship going forwards at a uniform speed. It is manifest that the sum of the forces driving her forwards is exactly equal to the resistance, or the sum of the forces acting in opposition to her motion, and yet she continues to plow her way through the water, for the forces that are opposing her motion exist entirely in virtue of the fact that she is going ahead. If the ship were made fast by a rope while the same force continued to drive her ahead, the strain on the rope would be exactly equal to the forces opposing her motion when she was free to move; but inasmuch as the strain on the rope is not due to the ship going ahead, there is nothing but a statical result.

On the same principle, centrifugal force is the resistance against alteration of the direction of motion, and arises entirely from the fact that the alteration is taking place; and also on the same principle it is equal to the force which is producing the alteration.

We must always bear in mind that we know nothing whatever about force but by its effects, of which the fundamental one is the sensation of pressure, without which it would be as impossible for us to form an idea of force as it would be for a man born blind to form a conception of color. But having by means of sensation acquired an idea of what force is, we can in various ways estimate the relative magnitude of different forces by comparing the effects produced. And whenever we find an effect produced which, according to our experience, force and nothing but force produces, I think we are justified in assuming that a force must have existed to produce the effect. Then, if we can show that resistance against change of velocity produces effects which nothing but force produces, we must come to the conclusion that resistance to change of velocity is a force, and if so, that it is equal to the force producing the change.

Take the following case. If a piece of heavy material, say a piece of lead, weighing 10 lb., is placed in the scale of an accurate spring balance, it will press down the scale till the pointer indicates 10 lb. I think it will not be disputed by Professor Tait, or perhaps any one else, that the pressure exerted by the lead on the scale, is a force downwards on the scale; for if not, what took the scale downwards till the pointer indicated 10 lb.? I do not say that that force *is* the force of gravity of the lead, for the same reason that when a weight is suspended on a chain the force exerted by the first link on the second *is* not the force exerted by the second on the third, but the one is the measure of and arises directly from the other; in like manner the pressure on the scale is the measurement and effect of the gravitating power of the piece of lead, and is all we know about that power.

Suppose now I wish to try experimentally what the effect of rapidly descending will have on the force of gravity, and I take the spring balance and piece of lead and go down a mine with it. When I am descending at full speed I find the pointer indicates exactly 10 lb. as it did before. A little while afterwards I experience a sensation as if I had suddenly grown heavier, and on looking at the spring balance I find that the lead appears now to weigh 12 lb. Of course the explanation is that the rate of descent is rapidly decreasing, and not that the lead had grown heavier. What then is the additional pressure on the scale? Is 10 lb. of the total pressure a force downwards on the scale, and 2 lb. of it not a force? Ten pounds of it is admitted to be a force and not to be the measurement of the force of the force of gravity of the lead, and 2 lb. of it is the measure of—what? Of the resistance which the

inertia of the weight opposes to the reduction of its velocity of course; but how can it produce this downward pressure on the scale if it be not a force? On commencing to reascend we get the same result; the force producing velocity upwards is exactly balanced by the resistance to the production of velocity, but the velocity keeps increasing all the same.

I will take one more case, but more apropos to the question of centrifugal force. Suppose a weight to be fitted to the arm of a wheel so that it can slide freely out and in on it, and let it be prevented from flying off the arm by a rim on the wheel; also let a spring be interposed between the weight and the rim. I will further suppose that when the weight is as far from the centre as it can go, the radius to its centre is one foot. If there is no power applied to the wheel the weighted arm will naturally find the lowest point and will press with its own weight, say, 10 lb., on the spring which will press with a force of 10 lb. on the rim. It will be impossible, I think, for any one to deny that the pressure on the rim is a force acting downwards on it, and therefore in a radial or directly outward direction from the centre.

If we now put the wheel in motion and make it run at about 54 revolutions per minute, then each time the weight passes its lowest point, the pressure on the spring and therefore on the rim will be equal to 20 lbs. Will half of that pressure be a force and half of it not a force, or will half of it be a downward force and half of it be an upward force? I see no escape from this absurdity but by admitting the existence of contrifugal force, and the non-existence of unbalanced force.

Let the wheel be placed horizontally, and be driven at the same speed as before; the force exerted on the spring will now be 10 lbs. constantly. Is any one justified in saying that in this case the spring is forcing the weight towards the centre, and deny the right of another to maintain that it forces the rim outwards? This is the ground that the anti centrifugalists take; they would say that the rim presses on the spring, and the spring on the weight forcing it out of a straight line. This is true enough, but the converse is also true, that the resistance being moved out of a straight line produces the pressure on the spring, and makes it press on the rim. It would be possible, however erroneously, to take the former view of the matter to the exclusion of the latter in such a case, as for instance, the moon revolving round the earth, where the centripetal force exists totally irrespective of the motion of the body, and is the originating cause of the curvilinear path. But if we are entitled to think in this manner, we are equally entitled to say, when the force is entirely due to to the motion that it is entirely centrifugal, and that there is then no centripetal force. Both views are equally wrong, and the only legitimate view seems to me to be the old-fashioned one, that when a body is moving in a circular path, the centrifugal force is equal and opposite to the centripetal force. Take the case of the common ball governor of an engine. If there is no such force as centrifugal force, what makes the balls fly outwards? —the force of gravity is pulling them out of the straight line, but surely the centrifugal force sends them out till gravity equalizes the outward and inward forces.

To me, the statement that a force cannot act without having an equal force to act against, is more like an axiom than a proposition requiring to be proved, for if it were otherwise it seems evident that a force could act on a vacuous space, which is a manifest absurdity. But it may be asked in the case, for instance, of gravity producing motion in a mass, what is the resisting force? Is inertia a force? Certainly not; but it is that quality of a mass which limits the rate at which motion can be imparted to it with a given force; which, in fact, prevents it from acquiring infinite velocity in an instant, and makes it possible to apply force to a body free to move. The resisting force only comes into existence when change of velocity begins to take place, and lasts only as long as change of velocity is taking place, in a similar manner to centrifugal force in relation to change of direction; and also in a similar manner to the case of the resisting forces to the motion of a ship. The latter is a case of uniform motion, and the former of variable motion, but the principle is the same. My contention is, that if we remove the support on which

a mass has been resting, we that instant bring another resisting force into play. The one may be called a statical force, and the other a dynamical force, the latter being the result of unbalanced statical forces. We cannot form a conception of this resisting force, but neither can we of the force of gravity, though the latter we consider the most natural thing in the world, just because we have been used to see and feel its effects. From what has been said it will be seen that if centrifugal force has to be given up, then the impact of a cannon-ball on a target is not a force, and the pressure of the exploded powder on the ball in the gun is not a force. These, and the great majority of what we have been accustomed to call forces, must stand or fall with centrifugal force.

THE TREATMENT OF IRON FOR THE PREVENTION OF CORROSION.

By Professor BARFF, M.A.

From "Journal of the Society of Arts."

While experimenting, two or three years ago, with my friend, Mr. Hugh Smith, on different methods for preventing incrustation and corroding of steam-boilers, I was led, through the failure of all the processes employed, to believe that, if it were possible to convert the surfaces of iron plates into the magnetic, or black oxide of iron, in such a manner that the particles of black oxide formed in the position of the original particles of iron could be rendered perfectly adherent to the iron surface, which does not become per-oxidized, and perfectly coherent with one another, the object would be effected. I do not intend to enter into the chemistry of the oxidation of iron to its full extent; it would take too much time, and it would rather tend to confuse than to enlighten those who are not well up in their chemistry, and would raise questions which would bring on prematurely a collision with the views of some of my brother chemists, which collision, under suitable circumstances, at some future time, not very remote, I look forward to with considerable satisfaction, as it will be the means of solving many phenomena which have never yet been explained. A piece of dry iron, its surface being polished, may be exposed for any length of time to dry air without rusting, but it begins to rust at once as soon as the slightest moisture comes in contact with it. We have to consider only two oxides of iron: one containing fifty-six parts by weight of the metal to sixteen parts of oxygen, and the other containing twice fifty-six parts of iron and three times sixteen parts by weight of oxygen. We speak of these oxides as the protoxide and sesquioxide, or as ferrous and ferric oxide.

Immediately the protoxide is formed, it being more moist, it unites with oxygen and becomes gradually converted into the ferric oxide. Now, let us suppose a moist iron plate to come into contact with oxygen. It is clear that the protoxide will be first formed, and this rapidly becomes converted into the higher oxide. Now, suppose you take a solution of the salt of the higher oxide and put into it metallic iron, in time, the air being excluded, this higher salt will become converted into a salt of the lower oxide. Let us now see how this bears upon the rapid oxidation of iron in the presence of moisture. We have seen that when oxygen comes in contact with moisture the first oxide is formed and becomes rapidly oxidized into the higher one. But this higher oxide is in contact with metallic iron, which will reduce it to the lower oxide, thus becoming oxidized by the oxygen which it has taken up from the higher oxide. You will now see clearly how it is that iron rusts throughout its whole substance with such rapidity, for the oxide of iron serves as a carrier for atmospheric oxygen to the iron to almost any depth. There is another oxide of iron, called the black or magnetic oxide, containing three times

fifty-six parts by weight of iron, and four times sixteen parts by weight of oxygen. Some chemists consider this oxide to be a sort of mixture of the two others, and they call it ferroso-ferric oxide; whether this be the case or not does not matter to us this evening. But it is a most important point for our consideration, that this oxide undergoes no change whatever in the presence of moisture and atmospheric oxygen. Nor does any temperature to which it can be exposed, in any of the ordinary uses to which iron is applied in the presence of moisture, either decompose it or produce its further oxidation. In every school where chemistry is taught, in the most elementary lecture on hydrogen, the pupils are told that if they pass steam over red-hot iron filings contained in an iron tube, they will be able to collect and burn hydrogen gas at the opposite end of the tube to where the steam enters. For a long time it was thought that the particles of black oxide formed by this decomposition of the steam were pulverulent, and could not be made to cohere into a solid mass. The result of a considerable number of experiments has been to prove that they can be made not only coherent amongst themselves but adherent to the body, and that both these produce a proper formation of this black oxide on the surface of iron plates; for, as I will show you later on, the oxidized surface of the iron resists for a long time, and more effectually, the rubbing with emery paper, than does the simple metallic iron itself, and that there is a very manifest difference between the ease with which a sharp rasp is able to cut away the surface of the iron, and the difficulty with which this black oxide is removed from the surface by that same instrument. The method which long experience has taught us is the best for carrying out this process for the protection of iron articles in common use, is to raise the temperature of those articles, in a suitable chamber, say to 500° F., and then pass the steam from a suitable generator into this chamber, keeping these articles for five, six, or seven hours, as the case may be, at that temperature in an atmosphere of superheated steam. I will presently call your attention to the diagram of the furnace and muffle which I have employed in all our later experiments, and in which all the specimens before you, which will be alluded to in this paper, were prepared. Differences of temperature are employed where different objects are to be obtained. If it be wished to act upon surfaces of polished iron or steel, it is desirable to let the temperature remain at 500° F., until the operation is completed. Articles coated in this way will not resist the action of continued moisture such as has prevailed for the last two months, when exposed out of doors; but they will resist the action of any amount of moisture with which they may come in contact in a house or building; and the reason of this will be very obvious, because only a thin film of the iron, on its surface is transformed into the black oxide. This I will explain more fully to you, when I call your attention to individual specimens. At a temperature of 1,200° F., and under an exposure to superheated steam for six or seven hours, the iron surface becomes so changed that it will stand the action of water for any length of time, even if that water be impregnated with the acid fumes of the laboratory. Before calling your attention to our failures and successes as they lie before you on the table, I will just allude to a few of the uses to which this process may be, as I consider, successfully applied—to water-mains, also to water-connecting pipes, as well as to the water-pipes used inside the house, which, in this case, would supplant their leaden predecessors. In this hall of hygiene, these words will, doubtless, sound as sweet music to the ears of many of those who have honored me with their attendance this evening. The greatest objection to the use of iron pipes for the supply of water in houses hitherto has been this, that by rusting they caused the first quantities of water drawn off in the morning to be dirty and turbid; now this will be entirely prevented, if the pipes be first exposed to the treatment which I have just explained to you—of course gas-pipes could with advantage be similarly acted upon—and as the surface, when oxidized, is harder than the natural surface of the iron, the friction of large bodies of water through the pipes, and the friction necessarily employed in fixing them in their places, would be much better resisted

than by the untreated iron itself. I cannot over-estimate the advantages which the employment of this process must confer on architects, who will be by it enabled to employ iron, whether wrought, or cast, much more largely, not only in the decoration, but in the construction of their buildings. Last summer, I was at a very large house in the country, where the entrance portico, some twenty feet high, was being painted and decorated, when one of the large plaster ornaments of the ceiling broke away from its holdings, and would have fallen to the ground except that it was caught by a workman. This ornament weighed not less than twenty-five pounds, and if it had fallen from this height upon the workmen below, it must have killed them. The ornament had been there many years, and was fixed up in the best method possible, it being supported and secured by iron rods. On examination I found that these rods were rusted through completely to the very center. I need not make any comment upon this, since I have been able to introduce you to iron treated in such a way that it will never rust. Of course if the process will answer for architectural ornaments, it will answer for statues, so that iron may be used instead of bronze, which will materially lessen the cost of casting statues, both in the material and in the expense of making the moulds. You well know that when a tinned saucepan is allowed to get dry on the fire and burns, as the servant calls it, that it is rendered useless until it is tinned again. Now, if such a saucepan be treated by the method I recommend it may be allowed to get red-hot without suffering injury, for the protection on its surface is produced at a red heat. We have experimented on some screws, hinges, locks, keys, bolts, with complete success. It has been suggested to me that the iron nipples used in gas-lights would not corrode, and would, therefore, be more useful, if submitted to this action of super-heated steam. Wherever iron is used, railings, street gas-posts, iron safes for keeping documents fire-proof and thief-proof, the framework of filters, tanks, cisterns for domestic and other uses, iron employed in the erection of temporary buildings—which, I flatter myself, if treated by this process, would become permanent buildings—all these, and many other applications of iron to the arts, would immensely gain by being submitted to this oxidizing action. I think I need hardly take up your time by enumerating other applications for the preservation of iron, for it appears to me that they would be commensurate with most of the uses to which iron is applied, save and except those where friction—such as that to which rails and iron wheels are exposed—would necessarily wear away the coating, as they wear away the material itself.

THE SEWAGE QUESTION.

By Mr. C. NORMAN BAZALGETTE.

From "Engineering."

The object of this communication was stated to be two-fold. First, to limit and define the proper application of the various systems introduced from time to time for dealing with the sewage of towns. Secondly, to direct attention to certain subordinate questions arising upon the practical operation of such systems. For the purposes of this paper, the following classification had been adopted: 1 Treatment with chemicals; 2. Application of sewage to land, including irrigation and intermittent downward filtration; 3. The dry-earth system; 4. The Liernur or pneumatic system; and, 5. Seaboard and tidal outfalls.

1. *Treatment with Chemicals.*—In this section of the paper, reference was made in considerable detail to the practical experience of the lime process at Leicester, Tottenham, Blackburn, and Birmingham; the A B C process at Leicester, Leamington, Crossness, Hastings, Southampton, Bolton, and Leeds; the sulphate

of alumina process at Coventry; the phosphate of alumina process at Tottenham, Barking, and Hertford; Goodall's process at Leeds; Bird's process at Cheltenham and Stroud; Dugald Campbell's process at Battersea; and Whitthread's process at Tottenham. It was stated, generally, that the experience of these processes was more or less identical with that which had been derived from Holden's, Hille's, Lenks', Suvern's, Scott's, and in fact all other methods in which, by the admixture of chemicals, it was sought to effect the purification of sewage by the precipitation of the dissolved and suspended impurities, and the ultimate realization of the precipitate in the form of a manure. This experience, coupled with certain opinions of Professor Frankland, Mr. Krepp, and Dr. Corfield which were cited, was relied upon as establishing the following conclusions: That no chemical process could efficiently deal single-handed with sewage, but must be assisted by subsequent natural or artificial filtration of the treated sewage, and therefore no chemical process *per se* should be adopted for the purification of town sewage. The principal objections to chemical processes, which appeared upon the experience of the places where they had been adopted, and upon which this conclusion was founded were, inefficiency of treatment, cost of treatment, and difficulty of manipulating the accumulations of sewage sludge.

2. *Application of Sewage to Land.*—The author first considered whether sewage could be made to yield an agricultural profit. The Parliamentary return of 1873 was referred to, and the financial position of the Warwick farm was specifically examined. The question was also raised, whether sewage possessed any fertilizing value beyond ordinary water for the purposes of irrigation, and the experience of the Barking farm having been appealed to upon this point, the conclusion was laid down, that no profit ought to be expected from the cultivation of crops by sewage irrigation. The next point discussed was whether any definite standard could be laid down as to the proportion population should bear to acreage in the practice of irrigation, the proportions exhibited by eleven towns being referred to, and it was determined that it was impossible to frame a specific rule. The theory of intermittent downward filtration was then investigated, as based upon the laboratory experiments of the Rivers Pollution Commissioners; and it was argued that the proportions which they had affirmed population might bear to acreage, ranging in the case of one acre drained six feet deep from 2000 to 3300 persons to the acre, were too high, and were not justified by the experiments. The practice of downward filtration at Merthyr was next referred to, and it was shown that the extent of its practical operation there had been exaggerated, and that the results confuted instead of confirmed the proportions of the Rivers Pollution Commissioners. The experience of Walton and of Kendal was also reviewed, and the following general conclusion completed this section of the paper: That where land could be acquired at a reasonable rate, irrigation was the best and most satisfactory known system for the disposal of sewage, but that intermittent downward filtration might be practised where the necessary surface area for broad irrigation could not be obtained. Experience, however, showed that the permanent proportion of population to acreage, where land was drained six feet deep, should in no case exceed 500 or 600 persons to an acre.

3. *The Dry-Earth System.*—The applicability of this system to towns was next considered, and it was shown that it must be supplementary to, and not substitutive of, a water-carriage system, thus enormously increasing the cost of making sanitary provisions for towns. The effect of its introduction into the metropolis, as a test case, was illustrated by figures, to prove that it would be superfluous, costly, cumbrous, and impracticable. Indeed, its applicability became diminished in the inverse ratio to the increase of population, to which it was proposed to apply it; and though it might be occasionally used with advantage in hamlets or detached buildings and institutions, it was unsuitable for the wants of towns.

4. *The Liernur or Pneumatic System.*—A description of the mechanical characteristics of this system was first given, and then the experience yielded by its operation at Leyden, Amsterdam, and

Dordrecht, was specifically analyzed. It was suplementary to, and not substitutive of, a water-carriage system, extremely costly, and its mechanism was complicated and liable to get out of order. The accumulation of sewage residuum in the central reservoir, and its subsequent decanting into barrels, were operations which could not fail to be objectionable and offensive. Its appliances were therefore not suitable for a high-class community, and no return from the manufacture of "poudrette" could be expected. In conclusion, it was urged that the system was of such a character that, though it might have a partial province in the tide-locked cities of the Hague, where no system of sewerage was available, it should never be imported into an English town.

5. *Seaboard and Tidal Outfalls.*—The first point considered was the return of the sewage of seaboard towns upon the beach; and it was maintained that where care had been taken to determine by float observations the force and set of the currents to which the sewage was to be committed, there was no difficulty in preventing such a result. The sea constituted the most natural and economical outfall for the sewage of towns situated upon it, and such means of outfall should be adopted. With regard to sewage outfalls upon the tidal portions and estuaries of rivers, there ought to be, arguing from the experience of the metropolitan outfalls, and assuming that proper precautions were taken in the selection of the outfall, and the exclusion of silt from the sewers, no danger of the silting up of the navigable channel.

THE GRAM MAGNETO-ELECTRIC MACHINE.—M. Tresca has made a careful measurement of the power required to drive a large and a small gram machine and compared the result with the light generator. A photometer disk was used, of which one portion was illuminated only by the electric light and an adjacent portion only by a carcel burner consuming forty grams of oil per hour. Much trouble was experienced from the difference in color of the two lights, and the equality was best obtained by interposing two plates of glass, one of light green and the other of light pink. Owing to irregularities in the carbons the light continually underwent irregularities sensible only to the photometer. The light of the larger machine was placed forty meters from the disk and the burner moved until the square of their distance should be as 1850:1, which was about the mean ratio of the two lights. When the two portions of the disk appeared equally bright the observer gave a signal and instantly the power and velocity were observed. The larger machine had a length of 80 centimeters, width 55 centimeters, and height 58.5 centimerers. The average number of turns per minute was 1274, and the work 576 kilogrammeters or 7.68 horse-power. The light being 1850 burners, would equal .415 of a horse-power per hundred burners, or .31 kilogrammes per burner.

The smaller machine had a length of 65 centimeters, breadth of 41 centimeters, and height of 50.6 centimeters. It made 872 turns per minute, and gave a light of 302.4 burners. This required 211 kilogrammes, or 2.8 horse-power, equivalent to .92 of a horse-power per hundred burners, or .69 kilogrammes per burner.

The consumption of oil to produce a light equal to that of the larger machine would be about 71 kilogrammes per hour or 194 cubic meters of gas. The cost of the oil would be therefore in Paris about a hundred times that of the electric light, or that of gas fifty times, to produce the same light. The comparison with the smaller machine would be less favorable. The carbons for the larger light had a cross-section of eighty-one millimeters, and the ordinary consumption was a little over a centimeter in length per hour.

—*Comptus Rendus.*

MAGNETIC SEPARATOR.—We have alluded in former reports to several machines employed in separating iron from copper filings and in concentrating the magnetic sands in Canada and elsewhere. We now learn that the machine invented by M. Charles Vavin is employed in concentrating the iron sands of the Island of Reunion, the pure magnetic particles being separated on the large scale and exported as iron ore to France.

IMPROVEMENTS IN THE MANUFACTURE OF POWDER.

Translated from "Revue Industrielle."

THE manufacture of explosive substances employed in the various industries, has within a few years made considerable progress. The most important improvement is certainly that of Dynamite, the invention of M. Nobel. This chemist in finding a simple method of depriving nitro-glycerine of its more dangerous qualities, at the same time preserving its high explosive power, has opened to inventors a new method, and suggested the creation of a series of products in which nitro-glycerine enters in greater or less proportion.

It will be sufficient to cite among others Hercules powder, in which nitro-glycerine is associated with the elements of ordinary powder, or with potassic chlorate; Ohlsen's and Norbin's powder, in which it is united with ammonic nitrate and wood charcoal; the varieties of lithofractors of which nitro-glycerine is the active principle; dualine in which a union is attempted of pyroxiline, nitre, nitro-glycerine, etc.

Notwithstanding the improvements in the preparation of dynamite, its manufacture must still be considered quite dangerous, and it requires moreover the employment of substances comparatively expensive, such as glycerine free from lime; concentrated nitric acid etc., all of which contribute to raise its price considerably above that of common powder.

Although much to be preferred in extensive works, where it can be brought to bear upon the harder rocks in water-bearing or submarine strata, dynamite loses a portion of its economical advantage when in the presence of rocks of medium hardness, where the properties of common powder permits the latter to be used with success. A preference for common powder is also explained by the disinclination of the workmen to adopt any innovation, even the best, and by the bad reputation caused by late accidents, although chargeable only to carelessness of miners.

M. Nobel has perfectly comprehended that notwithstanding the excellence of dynamite, it would have to contend against prejudices and established customs. So in 1870 he patented the employment of explosive compounds, consisting largely of nitrates, and differing from powder in the introduction of a certain quantity of nitro-glycerine.

When any powdered nitrate, such as nitrate of potassium, of sodium, of barium or of lead, is intimately mixed with carbon or any substance containing carbon or a hydrocarbon, such as resin, sugar, amidon, there results a mixture which if hermetically closed in by strong resistances, burns too quietly to be considered an explosive compound. But a small addition of nitro glycerine intimately mixed with such a preparation, in such manner as to form a layer upon the separate grains, will induce an instantaneous combustion throughout the mass and the consequent development of intense heat.

Considering the nature of such a mixture, it is evident that the proportions of the several ingredients may be largely varied without other effect than to increase or diminish the explosive power.

M. Nobel gives the composition of mixtures which serve as types of those of great strength and presenting the greatest security:

	1st type	2d type
Barium Nitrate.....	68	70
Charcoal.....	12	10
Nitro-glycerine.....	20	20

The addition of five to eight parts of sulphur to either of these mixtures augments slightly the power but diminishes the security.

Among the objections urged against dynamite, the most serious and the best founded relates to the exudation of nitro-glycerine from the grains saturated with it, and its gathering into little drops of which the least shock determines the explosion.

In 1873 M. Nobel secured a new patent designed to avoid this grave inconvenience. There were already a number of patents for explosive compounds made by mixtnres of nitrates of sodium, potassium, barium, ammonium, lead etc. carbonaceous matters, bituminous coals, or hydrocarbons; nitro-glycerine.

M. Nobel knew that in mixing a hygroscopic nitrate (sodium or ammonium nitrate) with any carbonaceous matter such as resin, sugar, or amidon, and with nitro-glycerine, if the compound were exposed to a humid atmosphere it would soon absorb water in which the nitrates would be dissolved, and thus exuding would cause the nitro-glycerine to exude also. But if all or a part of the carbonaceous matter is replaced by paraffine, ozokerite, stearine, napthaline, or any other fat substance which is solid at ordinary temperatures, and which will make an intimate mixture with the nitrates when reduced to fine powder, each grain of hygroscopic nitrate will become covered with a layer of the fatty substance, which will protect it from the action of the humid atmosphere. Furthermore the presence of paraffine or an analogous body prevents the exudation of nitro-glycerine even under the prolonged action of water.

It would be useless to attempt to give the composition of the numerous explosives, and two formulas only are therefore given, and which may serve as types:

Sodic Nitrate	69
Paraffine	7
Carbon	4
Nitro-glycerine	20
	100

Ammonic Nitrate	75
Paraffine	4
Carbon	3
Nitro-glycerine	18
	100

The patent specifies the employment of any substance greasy or glutenous to act in the double capacity of preventing the action of humidity upon the nitrates, and the exudation of the nitro-glycerine.

It has been deemed necessary to present the foregoing facts, before proceeding to explain the improvements covered by a patent of M. Courteille. His invention consists: 1st, in a modified form of blasting powder; and 2d, in a process of manufacture which we will briefly describe.

The principle of the invention consists in combining the ordinary elements of powder in the course of manufacture with the constituents of nitro-glycerine, in such manner that at the completion of the process there shall result a relatively large amount of powder holding a small amount of nitro-glycerine, completely absorbed and rendered stable by the materials of the powder.

The essential property of this compound is that it combines the properties of powder and of nitro-glycerine; not being capable of explosion in the open air, neither by friction nor percussion; only when enclosed and under pressure.

In its manufacture, the elements of both these explosives are brought together. First the nitrates of sodium or potassium or a mixture of them; sulphur, carbon in any convenient form, such as peat, coal, wood, charcoal or lignite. Then added to these constituents are the elements of nitro-glycerine, sulphuric acid, nitric acid, and oils or resin, such as paraffine, coal-tar etc.

The proportion of nitro-glycerine or nitro-resine is to be varied according to the quality desired in the explosive; to be increased when a destructive effect is to be produced, and diminished if only a displacement of material is desired.

The best method of preparation is the following:

Take fourteen to twenty parts of peat absolutely dry, and mix with three to five parts of oil or resin; add then from three to fifteen parts of acid mixture (two parts sulphuric to one part nitric); then mix eight or ten parts flowers of sulphur, with sixty or seventy parts of a hydrated solution of potassium or sodium nitrates or a mixture of the two. Put the whole mass in a steam-drying apparatus and stir vigorously for an hour, keeping the temperature between 101° and 110° C. Then gradually lower the temperature to 65° to complete the drying process. A small amount of remaining moisture produces the best results.

The inventor recommends for a mixer a well-known contrivance, consisting of a solid spindle working through a hollow one, each furnished with wooden arms, and made to turn in opposite directions. He also recommends that the temperature during trituration be maintained between 101° and 110° C, as it is manifestly important to keep the heat above the boiling point of water, and below the melting point of sulphur.

The acids of the mixture should be in

excess of the resinous and oily matters employed, so that none of the latter be left unaltered in the final product.

The advantages of this powder are;—its explosive force, which is from three to five times as great as that of common powder; the safety of its manufacture and use; and finally its excellent action. It costs much less to make than common powder, and the addition of the nitro-glycerine is effected without endangering life and property.

ON THE SYSTEMS OF CONSTANT AND INTERMITTENT WATER SUPPLY, AND THE PREVENTION OF WASTE.

By GEORGE FREDERICK DEACON, M. Inst. C. E.

Minutes of Proceedings of the Institution of Civil Engineers.

It has always been the custom to regard modern systems of drainage and water supply as great sanitary benefits; and if the state of things which they have induced be considered simply in relation to that which, without them, would exist in a densely-peopled district, the estimate is correct. But in the search for personal comfort in matters of household arrangement, the system of mere external drainage and water supply, for ages thought sufficient, was changed to one in which arterial connections were introduced between the dwellings of hundreds of thousands of people.

The altered arrangement had its obvious benefits, but, like almost every change in the manner of applying any scientific principle, or in the details of an invention, it developed new defects, which were either not noticed, or, if regarded at all, were treated as comparatively unimportant. These defects, however, are by no means necessarily inherent in the altered system, but are a consequence of the utter thoughtlessness with which the details in connection with dwelling-houses have in many instances been carried out. It is useless to inquire whether those who so readily caught at the idea of bringing drains and water pipes into more convenient places, ever contemplated the magnitude of the evil, which on the introduction of such new channels for the spread of epidemic disease, would become possible. Certain it is, that if any had the knowledge of the results which might ensue they were powerless to ensure throughout the country the exercise of those precautions which could alone render the system safe. The public drainage and water supply may generally have been executed under the best advice of the day; but, owing partly to the want of parliamentary powers, partly to the difficulty of supervizing in detail the work of speculative builders, partly to the fact that the control of the engineer was no longer thought necessary when the doors of dwelling-houses were reached, and partly to an abuse of the feeling that every Englishman's house is his castle—the strange incongruity has arisen, of works intimately affecting personal health and comfort, devised in a manner so crude, and indicating so little thought and intelligent action, that, when contrasted with the comparative perfection of those arrangements which might at a trifling extra cost have been adopted, the anomaly appears almost unaccountable.

Unexceptionable house drainage, however seldom realized, is generally regarded as indispensable; but the necessity for preventing waste of water is far less obvious. The consumer recognises in such prevention no direct sanitary benefit, and so takes little pains that the water fittings shall be of the best possible construction, and the pipes of the required strength; and although on the part of those who supply the water great alarm is often manifested, the conditions required to prevent leakage, even when no substantial impediment to its prevention exists, are often entirely disregarded. It might be supposed that water companies would be so influenced by the risk of increased future expenditure, that they at least would resort to all

practicable means for the prevention of waste; but, at the present time, many of those companies are allowing water to be laid on at high pressure for the supply of service pipes and fittings such as must inevitably, in the course of a few years, lead to great and continuous waste; and if this obtains under the control of bodies whose pecuniary interests are so intimately affected, it is easy to understand that the laxity in the case of many water committees of corporations and local boards is equally great.

When conditions such as these prevail, it is not strange that the state of pre-existing fittings and pipes is almost wholly neglected. There are but few towns, except those in which a water supply has only recently been introduced into the houses, which do not present the most incongruous combination of pipes and fittings of every kind in use since, or even before, the abandonment of those hollow trunks of trees which formed some of the earliest water mains, and many miles of which have recently been dug up. Between such antiquated appliances, and the pipes of the great strength and fittings of the comparatively perfect mechanism now known to be necessary to resist the influence of frost, surface traffic, and increased internal pressure, the step is a wide one; even the very existence of many pipes is often unknown. It is not surprising therefore that under both roads and houses there are innumerable sources of continuous waste, which no ordinary inspection, however careful, can detect. Exhaustive reports have been prepared by engineers; Parliamentary Committees and the Board of trade have inquired into the subject in the most thorough manner, and in a few towns complete success has attended the endeavors to prevent waste; but all this has done little to relieve the feeling of comparative helplessness which so generally prevails.

Under these circumstances the Author submits the following remarks on the subject generally, together with the results of his experience in the matter, gained in connection, not indeed with the metropolis, but with a town of the first importance, the water supply of which, having been changed from constant to intermittent service, has been regarded as a most unfortunate example of the evils resulting from waste of water.

2. SOURCES OF WASTE, AND EFFECT OF INTERMITTENT SERVICE ON THE SUPPLY.

The waste of water may be divided into two classes:

1st. Continuous or hidden waste, being that which flows from pipes and cisterns below ground, and sometimes by hidden pipes from cisterns above ground.

2nd. Discontinuous or superficial waste, being that which arises from defective fittings above ground, or from taps and valves temporarily left open.

In addition to these sources of waste there is frequently a considerable loss of water in its application to useful purposes; but as any attempt to induce a spirit of parsimony in this respect is reprehensible, it would be an error to treat the loss thus occasioned as one for the prevention of which it is desirable to adopt active measures. The expression, "water used or consumed," will therefore, in this Paper, include such as is drawn off by hand, whether actually applied to useful purposes or not.

Out of every 100 gallons of water passing into a service main during twenty-four hours, it is not unusual for 35 gallons to be lost by continuous or hidden waste, and 35 by discontinuous or superficial waste, while only 30 are drawn off for use.

This being the mode of disposition of the water under constant service, it is obvious that, if the duration of supply to the main in question were reduced to twelve hours a day, half the water hitherto wasted, or in this instance 35 gallons out of every 100, would be saved, less the waste from fittings connected with cisterns, which would continue until the storage was exhausted. But this would not be a correct measure of the gross reduction in the volume of water taken, as experience shows that the consumers would, while the water remained on, draw more freely than before, and would generally store as much of it as possible until the supply valve was again opened.

Thus the system of intermittent supply increases to some extent the use, or rather misuse, of water, reducing at the same time the most important element of waste nearly in direct proportion to

the reduction in the hours of supply, and as it is the most obvious method by which the loss of water may be limited, the expedient has frequently been resorted to.

3. OBJECTIONS ON SANITARY GROUNDS TO INTERMITTENT SUPPLY AND WASTE OF WATER.

When water is cut off from a district, the mains and pipes discharge themselves chiefly through leaks at low levels. Air rushes in from defects at higher levels to fill the space lately occupied by the water, or, if an opening to a pipe happens to be beneath a liquid, that liquid is forced into the mains by atmospheric pressure.

In most towns defects of this nature are probably as numerous and important as in Liverpool, where open ends and large holes in water pipes have been found in direct communication with the sewers and drains, and in such relation to the system of mains and pipes that an indraft of sewer air must inevitably have taken place each day during intermittent service, with which air the water first drawn in the morning must have been charged.

In the poorer districts of many towns water is laid on to closets by pipes direct from the mains. Under intermittent supply the basin cannot, therefore, be flushed out except while the water is on. The more frequent stoppage of the drains follows of necessity, and the pan often remains charged in consequence. The supply pipes are in many instances commanded only by common cocks, which are liable to be left open, and in such cases there must be an indraft from the closet pan each time the water is cut off from the district. These evils, which any person interested in the details of the distribution of water may ascertain, deserve the gravest consideration.

The system of compelling the poorest classes in overcrowded and unhealthy dwellings to store water for fifteen or twenty hours out of the twenty-four—as is frequently the case—in such inadequate vessels as they may be able to provide is also an evident sanitary evil, and is, in the Author's judgment, a most fertile element in the production of intemperance. The storage of water in cisterns for drinking and culinary purposes is most objectionable, much more so than is generally understood. Such cisterns are commonly found in close contiguity to, or even connected by a pipe with, the drainage system of the house. They are often uncovered, and so become the receptacles for decaying animal matter; frequently, too, they are exposed to the direct rays of the sun, and become polluted with organic growth; while, when containing water from some sources, they are often the cause of lead poisoning. The Author has in numerous instances found the overflow pipes from covered cisterns for storing drinking water actually connected with the soil pipes below the traps, so that the surface of the water was in direct contact with air from the house drains and sewers.

Under the intermittent system water for drinking is necessarily subject more or less to these conditions: under the constant system it may be drawn direct from the main, and the cistern may be retained for other purposes.

But, apart from all considerations of the intermittent service which it so often induces, the waste of water is harmful in itself; although some influential men advocate a supply of water so abundant that it may be wasted to an unlimited extent, on the plea that it flushes the sewers and teaches people to be cleanly. But these impressions are not borne out by the facts. Of all the water wasted a large portion does not find its way direct to the drains. Throughout the older parts of Liverpool, for example, even during a dry summer season, and although the basement of every house had been drained, the cellars and the soil around were in most cases completely saturated with water from the town supply. This was their state under intermittent service, and if constant service had been a possibility without a reduction of waste, its blessings would have been greatly alloyed by the augmentation of a condition which could not fail to exercise a prejudicial influence on the comfort and health of the people.

In isolated instances, and for a short space of time, some individual drain may be benefitted by the excess of water; but by the larger portion of the waste, viz., that which finds its way through the ground, and thence through the

brickwork, injury is done to the structural condition of the sewers, and to their effective action.

The most enthusiastic advocate of superabundant supplies would scarcely suggest that a larger quantity than 100 gallons per head per day should be given; but assume that 100 gallons per head per day, in excess of the water used, flow continuously down a single house drain. Suppose there are ten inmates, a number far above the average for each house, there will be wasted in twenty-four hours 1,000 gallons, a quantity which could not have the smallest beneficial influence upon any drain. It would be a mere dribble, such as would pass from a pipe, under ordinary pressures, through a hole the size of a pin's head.

The main sewers and outfalls in the lower parts of towns, where the quantity of water is usually too great, receive the aggregated driblets, and moreover, as in those parts the pressure in the mains is greatest, they receive the larger portion direct, and not after passing through the branch sewers above, to which they might in isolated cases be of some benefit.

Happily the cleanliness of branch sewers and drains is not dependent upon the waste of water. It is the result of experience that the sudden discharge of 2 gallons of water through a soil drain each time that drain is made use of is thoroughly effective in cleaning it, and this sudden flush is provided for by all properly drawn-up regulations as to water fittings. Further, at the heads of all branch sewers in towns flushing chambers for intermittent action may be constructed which will give a perfectly satisfactory result with an expenditure of water no greater than 1 quart per head per day.

The advocates for carelessness in the distribution of water may find numerous examples of the disastrous influence of too strong a feeling of security in abundant supplies. That such supplies are desirable cannot be doubted, but it is equally true that they should be conserved with the greatest care for the reduction of rates, the cheapening of manufactures, the systematic and intermittent purifying of sewers and streets, and last, but not least, to provide ever-flowing fountains among the dwellings of the poor.

4. PARLIAMENTARY POWERS GENERALLY FOUND NECESSARY TO PREVENT WASTE.

Now although intermittent supply is the easiest method of limiting the waste of water, it would probably have been seldom resorted to had not the authorities believed that they were powerless to contend by any other means with the growing waste. Nor has an examination of the methods adopted in those towns which have been most successful in preventing waste, often done more than suggest a remedy which has generally been regarded, by the representatives of the ratepayers at least, as worse than the disease. To the reports made by Dr. Pole, M. Inst. C. E., to the Board of Trade and Home Office in 1870 and 1871 on the constant service system, the Author is indebted for much of the following information concerning the change from intermittent to constant supply in two of the towns in which that change has been successfully carried out.

In 1850 the Norwich Waterworks Company obtained an Act incorporating the Waterworks Clauses Act, 1847, which bound them to give a constant supply. When, in 1851, this condition was complied with, and the pressure of water increased to that due to the height of the new reservoirs, the old mains gave way to such an extent as ultimately to necessitate their entire renewal.

In this manner it is probable that the waste was almost wholly confined to the private pipes and fittings, but the means which the Company possessed to control that waste were limited by sections in the Waterworks Clauses Act, which gave power to enter premises between 9 A.M. and 4 P.M., to ascertain if there were any waste or misuse of water; also power to impose a fine not exceeding £5 for any wilful act whereby the water might be wasted.

Such precautions as these clauses rendered possible were taken by the Company; but in Norwich, as everywhere, they proved almost abortive; and about the year 1859, the expenditure of water being 40 gallons per head per day, the Company were compelled to revert to intermittent service.

In the same year, acting under the advice of Mr. Hawksley, Past-President Inst. C.E., the Company obtained an Act to enable them to prescribe the nature in detail of all fittings and pipes and the work in connection therewith, and to interdict the use of any existing fittings and pipes which in their judgment might tend to waste.

The consumers had now no option but to abide by such regulations as the Company chose to make with reference to the fittings and pipes, and in a few years the consumption of water was reduced from forty gallons to about fifteen gallons per head per day, while constant service was restored. It must, however, be borne in mind that these results were obtained, first, by the renewal of the public mains, and, secondly, by the exercise of the almost unlimited powers vested in the Company.

In Manchester, the Corporation, having adopted the Waterworks Clauses Act, and received for the first time in 1851 an abundant supply of water from new works, gave a constant service, and were enabled to continue it. As in the instance of Norwich, however, the repression of waste was attempted under the Waterworks Clauses Act, 1847, and failed. In 1858 the Corporation, therefore, obtained an Act empowering them to refuse to supply water to any apparatus not so constructed as to prevent waste. But, inasmuch as no authority was named in the Act to settle the repeated disputes as to whether the fittings were so constructed as to prevent waste, this clause proved of little value; and in 1860 they again went to Parliament, and obtained clauses which were in effect similar to those already referred to as having been granted about the same time to the Norwich Waterworks Company; while in 1867 some minor additions were made to the Act.

By means of the great powers over the consumer's fittings thus placed in their hands, the Corporation of Manchester have not only been enabled to maintain constant service, but to prevent waste to an extent sufficient to retard considerably the rate of annual increase of consumption.

These cases are examples of the difficulties to be encountered, and of the ineffectual efforts to prevent the waste of water, except in a comparatively slight degree, without almost absolute control over the consumer's fittings and pipes. The provisions in this respect of the Waterworks Clauses Act have not, it is believed, hitherto proved sufficient in any town to enable constant service to be given and maintained, except with a ruinous expenditure of water.

The powers, however, to control waste possessed by the Corporation of Liverpool are, with the following exceptions, limited to those given by this Act.

1. In 1847 it was enacted that the Corporation should not be compelled to supply any water-closet, or the apparatus or pipes connected therewith, which had not been constructed in a manner approved of by the Council.

In an Act obtained in 1850, a clause was introduced inflicting penalties for wilful waste of water.

By the Act of 1862, powers were obtained to cut off the water where anything in contravention of the provisions of the Waterworks Acts was done to lead to waste; and further, it was provided that any person wrongfully suffering any fitting to be so used or contrived as that the water was, or was likely to be, wasted, should forfeit a sum not exceeding £5, and should remove, replace, or alter, or permit to be removed, replaced, or altered, the objectionable fitting or pipe.

Although at first sight they may appear satisfactory, these provisions have been so difficult to work, that, except when waste could actually be proved in each individual case, they have never been enforced.

The difficulty of satisfying a magistrate that a fitting is likely to cause waste is very great; while the necessity for taking into court all cases of waste, in which it is desirable to bring any person under the operations of the last-named provision of the Act of 1862, renders the clause almost abortive. On the other hand, the power to cut off the water supply can only be enforced to a limited extent by a corporation who are guardians of the sanitary condition of the town; and even where this step is resorted to, its effect is to punish the tenant instead of the landlord, who, in houses above £13 rental, is responsible for the work.

The only proper control over fittings or pipes, which the Liverpool Corporation possess, is in connection with water-closets. That control is to a limited extent given, as already stated, under the Act of 1847, and in 1866 it was further provided that every water-closet or urinal should have an apparatus to prevent waste, to be approved of by the Corporation.

When compared with those provisions which had been found necessary to reduce the consumption to any considerable extent in some other towns, it is clear how very little real power over the consumer's pipes and fittings the Liverpool authorities possess.

5. PAST CONDITIONS OF SUPPLY AND DEMAND IN LIVERPOOL.

Before explaining the method lately adopted in Liverpool for the prevention of waste of water, and the results which have attended those measures, the conditions of supply and demand during the last few years will be briefly stated.

In the year 1858, water was for the first time laid on constantly to nearly the whole district, an area of sixty square miles, which the Corporation were bound to supply. The quantity available, under the most unfavorable conditions, was apparently ample, and it might reasonably have been expected to meet the demands of the increasing population for many years to come; but within seven years the people were again subjected to the intolerable annoyance of intermittent service. In August 1865 it was found necessary to reduce the service throughout the borough to twelve hours, in September of the same year to seven, and ultimately to three hours a day; yet, notwithstanding this, the mean consumption for the year was 25.01 gallons per head per day, not including trade supplies.

Various steps were then taken to obtain further supplies; and before 1870 the whole available yield was increased by about one-fifth, by sinking new wells and boring in the New Red Sandstone near Liverpool, and by the purchase of water at Rivington, hitherto given as compensation. Notwithstanding this, however, the average number of hours' service was gradually and of necessity decreased, while the consumption per head did not diminish; and in 1873 it was for the first time found necessary to place the Out Townships, containing 109,749 persons, under intermittent service.

A new supply had been talked about, but many years must elapse before the necessary works could be carried out, while any year might bring with it a dearth of water. Powers for such a supply could only be obtained on condition that constant service would be given; and it had been ascertained by experiments that the average constant service would not be less than 33.5 gallons per head per day, exclusive of trade supplies by meter. Even if the Liverpool people had believed that they were justified in asking for it, with the examples of Norwich, Manchester, and many other towns before them, the fact that so large a proportion of their present supply during twenty-four hours was utterly and harmfully wasted would have constituted a powerful argument in the hands of opposing counsel. The further prevention of waste was therefore regarded as a necessity; while the restoration of constant service, which, it appeared to the Author, ought to be given with the existing supply, was looked upon as a possible concomitant.*

Inasmuch as the great reduction which has taken place in the number of hours' service has been accompanied, in many instances, by an increase of the rate per head, it is not surprising that experiments to ascertain the amount taken during twenty-four hours' service should have indicated that, in 1873, 33.5 gallons per head per day, exclusive of trade supplies, were necessary for the constant service of Liverpool, showing that an increase of 5.2 gallons had taken place in nine years; and although it may not be right to charge the whole of that increment to waste, yet only a small portion of it can be due to extended legitimate use.

To these considerations should be added the fact, that during the same period the trade supplies, measured by meter, have greatly increased.

The number of hours' supply per day must have been still smaller, had not the mean rainfall for several years past been

*Tables are omitted for want of room.

above the general average, and this circumstance had no doubt engendered a feeling of false security, which an examination of the statistics will not justify.

7. EXPERIMENTS IN DISTRICTS COMMANDED BY ORDINARY METERS.

In 1872 the Author had made a series of experiments, with a view to devise means for discovering the locality of a larger proportion of the waste than the house-to-house inspectors were enabled to trace. In carrying out those investigations, the mains supplying fourteen districts in the most densely populated part of the town, containing in the aggregate about 31,080 persons, were provided with ordinary piston meters.

Special house-to-house examinations were made, and the superficial and generally intermittent waste thus discovered was checked; but, although in some districts a marked diminution in the consumption was at the time produced, the results in others were not satisfactory. It appeared, indeed, that in order to detect actual waste in all those fittings which were liable to lead to waste, and thus to bring them, within reasonable time, under the power of the Corporation, it would be necessary, even in this comparatively small section of the town, to employ a large number of inspectors. Further, it was not at all clear that, even if all the fittings liable to lead to waste had been renewed, a very large proportion of the total waste would be prevented.

In four of the districts, which fairly represent the whole, and contain in the aggregate 7,817 persons, it was found, before any special examinations had been made, that while the consumption during nine hours averaged 17.26 gallons; and during twenty-four hours 34.5 gallons per head per day, the rate of flow between 1 A.M. and 4 A.M. was as high as 29.8 gallons per head per twenty-four hours. Moreover, it was ascertained that the rate of this nocturnal flow was constant, while the flow during the day was an ever-varying quantity. This result had been anticipated. The steady flow during the night indicated the rate of waste, while the flow during the day was made up of the waste together with the unsteady flow, due to water being drawn for use. From these figures it may fairly be inferred that 4.7 gallons per head per day, or the difference between the nocturnal rate and the total supply, was approximately the legitimate consumption of water in those districts under constant service.

Assuming what in such districts is very nearly true, that the rate of continuous waste during the nine hours of intermittent service was the same as the nocturnal rate under constant service, the actual amount of that waste during the nine hours was found to be 11.1 gallons per head, while the water taken, being the difference between this and the 17.26 gallons, the total supply during the same time, was 6.16 gallons, or 1.46 gallon per head more than the legitimate use under constant service.

The house-to-house inspections, though persevered in for some time, produced but little effect, and it was obvious that some much less laborious method of detecting the particular fittings causing waste was necessary; while there was a strong impression that a large portion of the continuous waste must be ascribed to other causes than those which the inspectors detected.

The iron mains and lead service pipes in these four disticts were old and shallow, and therefore, before charging the private lead service pipes below ground with the fault, nearly all the public iron pipes and the whole of the public lead service pipes were renewed.

Experiments were then conducted in the following manner:

A district was divided into a number of blocks of houses, each block being placed in charge of a trustworthy man at the same hour, while the occupants were requested not to allow any water to be drawn until they received further notice. As an additional precaution, these men closed and fastened every tap, ball-cock, and other fitting, in order to prevent all use, and so far as possible all superficial waste. The meter resumed its steady rate of movement, and the quantity found to be still passing averaged 7.9 gallons per head per day; while in one of the four districts in question it was as high as 14.5 gallons. Outside each house, under the footways, the service pipes had been provided with stopcocks, and while the fittings were

still tied it was found that each individual waste could in nearly every instance be heard, when the street was quiet, by applying the turning key to the stopcocks and placing the ear upon it.

The application of this system of detecting waste has been long known, but it was found impracticable to carry it out successfully during the day, except as an experiment, because it was necessary in each case to tie up and watch the fittings, and because the noise of traffic rendered the sounding operations extremely difficult.

During the night these obstacles did not exist. So long as the Engineer or any of his chief assistants personally superintended the nocturnal work, the results were successful; and by counting the strokes per minute of the meter from time to time it was easy to ascertain the waste, which had been checked by closing those stopcocks through which water was discovered to be passing. The premises connected with such stopcocks being reported on the following morning, became the subjects of examination by the day inspector.

The advantages of the system were manifest, as the labor of discovering waste was reduced, the certainty of discovery increased, and the annoyance to conscientious householders by an ordinary house-to-house inspection removed.

By these means the consumption of water for the whole set of experimental districts, containing 31,080 persons, which under nine hours' service had been 19.5 gallons, and under constant service 33.5 gallons per head per day, was reduced under constant service to 13.3 gallons, and subsequently to about 12 gallons; while in one district of 2,134 persons it was brought down to 6.6 gallons, without altering a single fitting which had not been found to be actually wasting water.

A little consideration showed, however, that, in order to obtain such results over the whole district of supply, including a population of 630,000, in a sufficiently short time to ward off the expected dearth of water and to restore constant service, several districts must be inspected every night. The difficulty of efficient supervision would have been very great, and it would have been impossible to ascertain how far the closing of the stopcocks commanding wasteful property had been faithfully performed, as no record was preserved of the change in the rate of movement of the piston meter, except that returned by the inspectors themselves. The cost, too, of reliable piston meters and of the necessary chambers was at least £112 for each district of about 2,500 persons, while the expense of maintenance was considerable.

8. THE WASTE-WATER METER.

It was under these circumstances that the Author devised an automatic check upon the motions, at once of the night inspectors and of the water, the cost of which is comparatively small. The instrument known as the Waste-water Meter records the time and rate of flow through the main, after the manner in which an indicator diagram records space and pressure. The diagram drawn by the instrument distinguishes water wasted from water used. It registers not only the change of velocity, induced by the closing of any stopcock during the night, but the time at which the men commenced work, and the time at which they ceased work. Such an instrument would, if applied to all service mains, give the means, first, of ascertaining at any time the most wasteful districts, and thus enable the superintendent to concentrate the energy of his men on the streets which most require attention, and subsequently of insuring the efficiency of the examinations during which the waste is limited to a few premises only.

The apparatus has been described elsewhere, and was recently reported upon to the Liverpool Corporation by Mr. F. J. Bramwell, M. Inst. C.E. It is unneccessary, therefore, to do more than explain, that in the instruments now made the diagram is carried on a revolving drum six inches diameter, and is divided by horizontal lines into sixty-two spaces, each of which represents 100 gallons per hour; so that the maximum rate of flow capable of being registered is 6,200 gallons per hour. By vertical lines the diagram is divided into twenty-four equal spaces, each of which passes the pencil in one hour. In the actual sheets the horizontal lines are not quite equidistant.

9. APPLICATION OF THE WASTE-WATER METER SYSTEM.

Arrangement of Distributing Mains.—Although perhaps there are no features peculiar to Liverpool in the manner in which the distributing mains are laid, it is desirable to give examples of their arrangement before describing the details of the Waste-water Meter system.

The principal mains, are charged at all times. From them most of the fire hydrants and larger manufactories are supplied. The subsidiary, or service mains, vary in diameter from two to six inches, and are commanded by valves near their junctions with the principal mains. Of these valves there are about three hundred in the borough, and under intermittent service they were opened and closed every day.

Division into Waste-water Districts.—The existing arrangement of services for distribution is as far as possible adopted. Where, however, a service is too large, it is divided, or a portion is added to an adjoining district, while in some instances, where the services are too small, two or three are combined to form one meter district. Each case is considered in relation to the estimated population, and the anticipated rate of consumption per head, bearing in mind the fact that if the flow at any time exceed 6,200 gallons an hour the diagram will be imperfect. In some cases, after the waste has been greatly diminished on a service, an adjoining district is connected with it, in order to reduce the total number of meters employed.

The largest Waste-water Meter district in Liverpool contains 4,271 persons, the average maximum rate of flow being about 3,500 gallons an hour, and the mean rate per head per day 13.5 gallons.

The smallest meter district contains 870 persons, the average maximum rate of flow being 4,000 gallons an hour, and the mean rate per head per day fifty-one gallons.

The total number of meter districts on the 10th April, 1875, was 120, and the population under control 306,912; so that the average population of the districts was 2,557.

Fixing the Meters.—A district having been selected, a sketch is prepared showing the place the meter is to occupy. A single length of pipe is removed from the outlet side of the service valve, and connecting pipes are laid to beneath the nearest point of the footway and convey the water through the meter. The cast-iron meter cover is placed in the footway next to the kerb.

Diagrams of Initial Consumption.—After a meter has been fixed, readings are taken to ascertain the consumption of water in the district under the ordinary intermittent supply. The district is next placed under constant service, and the consumption ascertained under that system, before any measures for the prevention of waste are commenced. In twenty-four hours a complete diagram is drawn. If the meter is left for seven days, seven complete diagrams will be superimposed upon each other. In such cases the night consumptions are distinct; but the day consumptions, owing to the greater fluctuation, are generally more or less confused. It is therefore usual at first, when particularly accurate information is required, to remove the diagrams every second or third day.

Fixing Stopcocks.—The next step is to attach stopcocks to the lead service pipes of all premises, or blocks of premises, which are not already capable of being controlled from the exterior. The details of the stopcock and its cover are most important; and in Liverpool the existing plugcocks and defective covers have created much difficulty. All new stoptaps are on the screwdown principle, and the loose valve is so arranged that it cannot fall out when the stuffing-box is removed. The surface cover is fixed into the upper side of the flag with Portland cement. The hinged lid is round, and large enough for a man to pass his arm down. Though all new pipes are laid two feet six inches deep, the stopcocks are fixed just within arm's length, in order that a washer may be replaced without opening the ground.

Census Returns.—The number of persons included in the area under test is now to be determined. In residential property a fair estimate can sometimes be made on the basis of the Government census, or by analogy from actual numbering by the water inspectors in another and similar class of property. Where, however, as frequently happens, a satis-

factory estimate cannot be made, inspectors are sent to every building to inquire the number of occupants, and in these cases they also examine the fittings, and furnish a report.

Record of Results.—The population having been obtained, the diagrams already taken, both for constant and intermittent service, are worked out, and the results entered in a suitable book.

Night Inspections.—The intermittent and constant service diagrams having been obtained, and the forms filled up, the night inspections, which constitute important features of the system, are commenced. Each day the superintendent of the waste-water department examines the books, and ascertains the three or four districts in which the largest saving is likely to be made. He sends to the night-complaint office an instruction for those districts to be visited that night; and the waste-water meter inspector, having obtained the information, places upon the meters commanding each a new diagram paper, which he removes on the following morning.

At about 11 P.M. the inspectors proceed in pairs to their respective districts, where their operations are conducted in the following manner:

Each man takes one side of a street, raises the stopcock covers, and subjects the stopcocks, one by one, to a test, to ascertain if water is passing into the premises. If, on applying the turning key to the stopcock plug and the ear to the turning key, no sound is heard, the stopcock is turned until nearly closed; and if water be passing it will then, by reason of its increased velocity, make a sufficient sound to be heard.

In this manner the flow due to a quick dropping can be easily detected. If noise is heard with the tap open, the tap is at once completely closed, and tested again, when, if the sound is continued, there is evidence of waste on the street side of the stopcock.

It sometimes happens that a number of adjacent stopcocks sound both shut and open. In such cases the inspectors re-examine them, and are generally enabled to ascertain to which stopcock the leak is nearest. Having discovered this, they sound the flags of the footway, and the sets of the carriage way, and in most instances succeed in determining the exact position of the fault.

In other cases several consecutive stopcocks give sound shut or open, until one is reached which sounds when open only, and is most probable the cause of the whole group of sounds. It is closed, and the last stopcocks being re-examined are, if the sounds have ceased, cancelled from the notes.

The exact time of closing each stopcock at which the flow of water is detected is booked by the men, who are provided by the Corporation with watches for the purpose.

When the whole district has been traversed the inspectors retrace their steps, and open the closed stopcocks, after which they return to the office, which they generally reach before 6 A.M., and where they at once write, in copying ink, a report of their night's work.

At 9 o'clock on the same morning a press copy of a night report is ready for each day inspector, who, accompanied by a laborer to open the ground and trace underground leakages, proceeds to ascertain the cause of the noises reported by the night inspectors, and makes his report.

Where the leakages arise from defective private pipes or fittings, notices to repair or renew are issued, unless a new washer or an adjusted wire will do all that is required. In such cases the inspector performs the work free of charge. Defective public pipes are reported, and repaired by the Corporation pipe-layers. The diagram from each district, subjected to an examination on the previous night, is brought to the office daily, and by its aid, in conjunction with the night reports, the superintendent determines the time, within a few minutes, when the inspectors commenced their work and left the district in each case, the localities of all, and the amounts of the more important leaks which have been checked, the initial rate of waste, and the final amount to which the closing of a limited number of stoptaps has reduced that waste.

The diagrams are preserved for reference, in the event of failure to discover the cause of waste at any of the reported points.

Tests for Condition of Mains.—The diagram obtained after a night inspec-

tion in any district shows the lowest consumption reached by the closing of those stopcocks, past which a flow of water has been detected. If a margin remains which cannot be accounted for by such trifling leakages as may have escaped the notice of the inspectors, or by leakages reported on the street side of the stopcocks, a special experiment either by day or night is made by the chief inspector to determine the condition of the public pipes. All the stopcocks having been closed, the diagram is examined. If a steady line is being drawn, it is a proof of steady waste. If, on the other hand, the horizontal line is broken here and there by a vertical line, similar to that produced by opening a tap, there is conclusive evidence either that all the stopcocks have not been closed, or that some house services are not provided with stoptaps. In old districts, however great the care on the part of the men, some omissions are in the first instance almost sure to be made; but, with the help of the meter, every service is ultimately controlled.

When the existence of the so-called "public waste" has thus been determined, it can generally be localized by a careful night inspection in which all the stopcocks are closed and sounded. If this is not entirely successful, the sewers are entered, and the position noted of any undue flow down a drain or weeping through the brickwork.

The ground is never opened until the locality of the fault is determined, and mistakes are of very rare occurrence.

The experiments in the fourteen experimental districts commanded by piston meters were commenced in January, 1873, but it was not until October in the same year that the first Waste-water Meters were fixed. In its earlier stages the system progressed but slowly. During the last twelve months, however, it has gained in speed; and, except for the purpose of obtaining the census in some districts, the house-to-house inspections have been entirely abandoned, and no premises have been entered except those in which the night inspections had previously discovered the existence of waste, or to which the attention of the officials had been specially drawn. For some time past twelve meters, controlling about 25,300 persons, have been fixed each month; and it is found that with one chief inspector, twelve ordinary inspectors, six laborers, one Waste-water Meter inspector whose exertions are confined to the meter districts, and two inspectors to attend to special reports in other districts, it is easy to restore constant service to 5,000 persons per week, to sustain it in the old meter districts, and to maintain the average consumption over the whole area supplied at a constantly diminishing rate.

The recent progress of the system may be shown by comparing the circumstances of 1873, of 1874, and of 1875.

In March 1874 about 170,996 persons, and in March 1875, 373,500 persons in the borough were receiving constant supply.

During 1873 the maximum number of hours' service to the intermittent districts in the borough was 10½; the minimum, 7½; and the average, 10. Since then the average has been maintained at 10½ hours throughout the year.

In 1873 the Out Township supply to 91,835 persons was reduced for an average of fourteen weeks to 12 hours per day. Since then constant service has been maintained in the Out Townships.

At the beginning of 1874 the total population supplied was about 621,000. At the beginning of 1875 it had increased to about 632,000.

During 1873 the water taken for all purposes, excepting manufacturers' supplies, measured by meter, was 5,437 million gallons, or 24.42 gallons per head per day. During 1874 the corresponding quantity was 5,478 million gallons, or 23.8 gallons per head per day.

In March 1874 the rate of supply was about 24.21 gallons per head per day.

In March 1875 it was about 22.26 gallons per head per day.

Actual Saving of Water.—It is to be regretted that comparatively few persons apprehend the importance of the work in progress, with anything like the eagerness which would be evinced if the reduction in the rate of waste, instead of being chiefly manifested by the restoration of constant service, were shown in the actual figures of water saved, as would be the case if Liverpool had in 1872 been receiving constant supply.

Fortunately the meters give the means of determining the actual saving in

water, on the assumption that the town was receiving constant service from the beginning.

In nearly every waste-water district, the quantity of water taken, under the constant system, has been determined before disturbing the initial rate of consumption.

The number of persons to whose service the Waste-water Meter system has been applied in Liverpool, to the 10th of April, 1875, is 234,000, but many of the districts have as yet received little attention at the hands of the night inspectors. Taking that moiety of the districts to which attention has been chiefly given, the supply to 117,425 persons, which under constant service averaged 32.12 gallons, has been reduced to 15.97 gallons per head per day. If the whole population under treatment be considered, the rate per head per day to 234,000 persons, is found to have diminished from 28.89 to 16.47, which is equivalent to a total saving of 1,060,792,200 gallons per annum.

It is known, by experiments in a few isolated cases, that the waste in these districts may be still further reduced; but the Author systematically avoids occupying the inspectors' time in those portions of the town to which constant service has been restored, except so far as it is necessary to do so, in order to keep down the total consumption, as district by district the change is made.

In comparing the rate of consumption per head per day in Liverpool, with that of other towns, the following considerations must not be lost sight of:

Unlike most towns in which constant service has been given and maintained at a low rate of consumption, the inmates of four-fifths of the houses are provided with water-closets.

The term "domestic consumption" has been somewhat improperly applied and has a much wider signification than in most other towns, as it includes all supplies for sanitary purposes, and a large proportion for business purposes. In other towns it is usual to confine the term to the use of water in private dwellings only; and this difference would be equivalent, in most cases, to several gallons per head per day.

The system described has been tried in every class of property; and in some districts containing the best houses the saving of water has been quite as great as in any others. It is certain, moreover, that the most wasteful districts have not yet been reached, and there is every prospect of a rich harvest during the present year.

10. RELATIVE ADVANTAGES OF THE WASTE-WATER METER SYSTEM AND OF THE ORDINARY HOUSE-TO-HOUSE INSPECTIONS.

The policy of the present practice differs chiefly from that of the old system in four particulars:

1. By never employing inspectors in any but those districts in which the meter diagrams show that the best results are likely to be attained. As the following facts illustrate, this alone is a great desideratum.

Out of a hundred waste-water meter districts, several cases have been chosen in order to show how widely the consumption and degree of waste differ in districts of the same age and class, before any special work has been attempted in connection with them. The dark figures are cases of large consumption, and the light figures of small consumption in similar districts. (See table next page.)

In the examples given there was nothing whatever to lead to the belief that any of the districts in dark figures would constitute more fruitful ground for the labors of the inspector than those in light figures. Under the ordinary system both would have been examined alike, and the energies of the inspectors in a great measure wasted.

2. By never employing inspectors to examine any premises in which it has not been previously ascertained that waste is taking place.

Although, as already stated a great desideratum would be gained by adopting house-to-house inspection, but confining it to those districts only from which the meter diagrams show that the best results would be obtained, the advantage is greatly increased by abandoning regular inspections even in those districts, and by adopting means, such as have already been described, to ascertain in which premises waste is actually taking place.

With far better methods for detecting waste at their disposal than the house-to-

Number of District.	Population.	Consumption per Head per Day.		Remarks.
		Average.	Night Rate of Waste.	
		Gallons.	Gallons.	
270 and 271.....	1,319	40.6	30.4	Adjoining districts resembling each other in every respect.
258 and 264.....	2,778	11.8	7.3	
254.....	2,082	40.7	33.8	
266.....	1,661	14.67	9.1	Ditto.
259.....	2,765	16.0	9.13	
14.....	838	41.0	34.1	Similar class of property, but the district which gave the higher rate of consumption would have been supposed to be less wasteful than the other, owing to many of the fittings being of a better class.
18.....	4,012	15.8	12.5	
8.....	827	45.0	35.5	Districts in the same locality and property similar.
7.....	2,570	55.0	45.6	
15.....	2,377	13.6	8.5	
237.....	2,261	54.8	44.5	Ditto.
262.....	3,430	11.5	7.37	

house visitors have even possessed, the night inspectors of Liverpool do not, as a rule, find on an average more than about one leak at every tenth house.

The annoyance to the inmates, therefore, and the loss of time is in nine-tenths of the cases avoided, while the fact that in or under each house visited by the day inspector who follows up the night reports, a leak certainly existed on the previous night, is an incentive to the discovery of the source of waste which under the ordinary system cannot possibly exist.

3. By the detection of the more important, because the more continuous, sources of waste which are rarely discovered by house-to-house visitations.

It has been shown, that in some of the experimental districts a large portion of the waste was of a kind the source of which could not be detected by ordinary house-to-house inspections.

For the purpose of ascertaining the respective advantages as measured by the quantity of waste discovered by house-to-house visitors and by night inspectors, a number of districts in various parts of the town have been taken at random. The ordinary day inspections were made after the meters were fixed, and particular care was taken to insure their completeness, not only by examining the fittings and pipes thoroughly, but by testing the fittings by sounding. They show generally that the effect on the consumption thus produced, after subsequent examinations to ascertain that the defects discovered had been repaired, was to reduce the demand from a night rate of 20.6 gallons to 17.3 gallons per head per twenty-four hours.

Immediately the results of the day inspections had been ascertained, night inspections had been made, and it was found from the diagrams that a further diminution in the consumption of seven gallons per head per day was thus produced.

In other words, where the most careful day inspection reduced the consumption from 20.6 gallons to 17.3 gallons, a night examination, if substituted for it, would have reduced the consumption from 20.6 gallons to 10.3 gallons.

The same advantages are also shown in a striking manner by comparing the proportion of waste detected by the inspectors in the past with that discovered by the night inspections.

In 1863, when the proportion of inspectors to a million persons was 3.86, the proportion of the leaks discovered from defective fittings and pipes to the number of premises visited was 14.1 per cent.

In 1870, after the system had been in operation for seven years, and when the proportion of inspectors to a million persons was 17.12, the proportion of the

leaks to the premises visited was only ten per cent.

In 1872, when the proportion of inspectors to a million persons was 28.12, the proportion of the leaks to the premises visited was only nine per cent.

The special house-to-house visitations subsequently carried out under the more favorable circumstances in the districts referred to above, which are fair examples of the whole, when the proportion of inspectors to a million persons was 25.2, indicate that the proportion of the leaks to the premises visited was 12 per cent.; the diagrams at the same time showing that the repair of these leaks produced a reduction of 3.36 gallons per head per day in the night rate.

But in those very districts in which so high a proportion of waste had been checked, night examinations under the meter system, made immediately afterwards, detected an additional proportion of leaks to premises of 8 per cent., and an actual additional waste of 11.15 gallons per head per day.

An analysis of the returns of the day inspectors, made upon the reports from the nocturnal examinations, shows that the additional and much larger waste thus detected, though continuous, was attributable to causes which the house-to-house inspectors must necessarily have failed to detect.

A large proportion of the leaks thus discovered are due to defective pipes underground, and of these a few samples are exhibited, with notes as to the circumstances under which they were discovered, and the quantity of water which each will pass at 50 lbs. pressure.

4. By never renewing old mains except when the meter diagrams have shown that they are actually causing waste to an extent which justifies the expenditure.

In many waterworks the distributing mains are of all ages, and being, in a greater or less degree, constant sources of unseen waste, it is usual to spend a sum of money annually for the renewal of those which are believed to be the oldest, or which may have happened to show their deficiencies by leaks appearing above the surface of the ground. Only those who have tried it by absolute measurement can form any idea of the fallacy which commonly obtains in reference to this matter. In Liverpool it has been found that the number and importance of the defects in mains, which continue for years without giving any superficial indication of their existence, is very great. Experienced managers of waterworks have expressed the opinion, which was until recently shared in by the Author, that a main could not break across without revealing the fact in a short time. But the present system has brought to light many instances of broken mains in which the sharp edges of the fracture have been worn off in the course of years by water under a head of 100 or 150 feet. The other chief causes of waste from the mains are wooden plugs in old ferrule-holes, either driven out by the water or leaking, wooden plugs partially decayed, which were formerly used instead of iron caps on the ends of mains, and blown lead joints. On the other hand, most of the oldest mains, although they are fractured by the slightest disturbance, and are liable at any time to lead to great loss, are perfectly tight, and might remain so for years.

It is, therefore, an obvious economy to have a better means of ascertaining their condition, and thus to gain maximum and immediate results, by the prevention of waste, from money the interest or great part of which was formerly lost for years.

11. COST OF APPLYING THE WASTE-WATER METER SYSTEM.

Where stopcocks on the service mains do not already exist, either inside or outside the houses, it is obviously desirable that they should be fixed, if only to avoid the great inconvenience arising from having to cut off the water from a large district each time a repair to any pipe or fitting becomes necessary. In Liverpool the work of fixing stopcocks had been commenced before the meter system was introduced; but, inasmuch as stopcocks outside the houses are necessary for the complete development of the system, the cost of providing them will be considered here.

In Liverpool, as in most other towns, a certain number of stoptaps, but probably only about one-fourth of the number required, already existed.

The cost of providing and fixing each

new stopcock, including the surface cover, reflagging, renewing defective flags, and searching for the service pipe, is about 12*s*.

The average number of persons to each meter is 2,557.

The cost per thousand persons in a town in which stopcocks are already laid would average for the meter and fixing £14 7*s*. If, however, as in the case of Liverpool, the greater proportion of the stopcocks have to be charged to the system, the cost, including the meter, would be about £39 7*s*. per thousand persons.

The necessary plant having been provided, the operations for the prevention of waste immediately show a great reduction of expenditure.

The staff of inspectors has been somewhat reduced since the system was commenced, but there are no means of comparing accurately the water saved in each case, though it is obvious from a general comparison of the results under the two systems that a small fraction of the old staff would, under the district system, have performed more work, while no increase in the numbers of the original house-to-house inspectors could have effected the saving since obtained. A clerk's time for about four hours a day is occupied in computing the diagrams and recording the results, but the number of turncocks has been diminished by three, so that on the whole the staff has been reduced.

12. TREATMENT UNDER THE WASTE-WATER METER SYSTEM AFTER THE RESTORATION OF CONSTANT SERVICE.

It has been already stated in effect that during the last twelve months all pipes and fittings, with the exception of those actually leaking in the meter districts, or reported by the public, have been left to themselves. Notwithstanding the greater proportion of waste which those pipes and fittings therefore occasion, and notwithstanding the fact that in the waste-water districts it was necessary to save twelve or thirteen gallons per head per day on the initial constant supply before the consumption could be reduced to the same amount as the initial intermittent supply, it has been shown that a reduction in the total supply per head per day has been effected, and it has been further stated that the rate is gradually diminishing.

From these facts it is reasonable to conclude that when constant service has been everywhere restored, and the water saved, whatever it may be, is no longer absorbed in making up the difference between intermittent and constant service, but effects an immediate reduction in the total supply, the staff required to maintain or even to diminish the comparatively low consumption will be considerably less than at present.

From experience gained in the districts already under control, there is reason to believe that, in order to keep a proper record of the condition of the whole of the town, it will then only be necessary to take on the average one meter diagram for three or four days per month from each district. The total number of districts, including in the aggregate 632,000 persons, will be about 250, and the number of diagrams per day, including those from districts under examination, will probably not exceed 20.

Each day, two or three districts of maximum waste will be provided with new diagrams, subjected to night examinations, and when brought below the average consumption, replaced on the ordinary list, and only provided with new diagrams when the district of which each forms a part is visited in its monthly turn.

13. TESTING AND STAMPING FITTINGS, AND INSPECTING NEW WORK.

A very important section of the work in connection with the prevention of waste is that relating to the class and quality of the pipes and fittings fixed in new premises, and substituted for those which have been prohibited. However good the fittings may be, it is certain that, unless some supervision is exercised over them by the authorities, large numbers will become, and continue to be, defective. If, however, they are satisfactory to begin with, only trifling repairs, easily effected, will in the large majority of cases be necessary.

It has already been stated that the power of the Liverpool Corporation to prescribe the details of water fittings is very limited, and by way of insuring the execution of their requirements in cases of new fittings, they have adopted, with

some modifications in detail, the system of testing and stamping fittings, and of advertising the names of those plumbers who agree to conform to their regulations, which has been successfully applied by the Manchester Corporation; with this difference, however, that the latter body have parliamentary powers to license the plumbers approved of.

In March 1873 the Liverpool Corporation invited each plumber to sign an agreement, undertaking that all work performed by him should be in conformity with the rules and regulations of the Water Committee.

The Corporation, on their part, place the names and addresses of such plumbers on the waste-water notices, and recommended them in preference to others. Nearly all the plumbers whose names appeared in the Liverpool Directory signed the agreement, and in comparatively few cases have the rules been infringed.

The published regulations, drawn up in 1873, have recently undergone some slight alterations, which experience has shown to be desirable. In their modified form they appear to work well.

In the testing office a superintendent with one assistant and two boys are employed, whose duty is to examine each fitting superficially, and to ascertain that it apparently works satisfactorily. If a tap or ball-cock, to place it in jaws, provided with the means of supplying water at a pressure of 300 lbs. per square inch. If a regulating cistern or service-box, to ascertain by actual trial with water that the valves work truly, and that the delivery valve passes the 2-gallon flush in less than fifteen seconds. To weigh, gauge, or examine every part of each fitting, including valves, valve-seats, washers, valve-rods, and, in the case of alternating-valve regulating cisterns, to see that the valves have the prescribed lift, and that the lever has the prescribed play between the respective lifts. And to stamp those fittings only which are in all respects in conformity with the samples kept for exhibition.

Any person who desires to introduce a new description of fitting, or even a change in the form of an existing sample, is required to send it, not to the testing office, but to the Engineer. If after undergoing examination, and being subjected to such tests as are thought necessary, it is considered no less satisfactory in principle, quality, and workmanship than the general standard hitherto adopted, it is placed among the sample fittings, and may thenceforth, unless, of course, protected by patent, be copied by any other maker. No licence is given to the superintendent of the testing department to allow deviations in any respect from the adopted standard in each case.

At first sight, this great stringency may appear unnecessary. On the introduction of the system into Liverpool, some latitude was, however, given to the testing officer, and the consequence was that each maker endeavored to undersell his neighbour by trifling reductions in the material and in the quality of workmanship. Thus step by step the standard was reduced, until at last it became necessary to insist that no change whatever should be made, unless one of the altered fittings was accepted as a standard sample.

The present system allows fair competition, and encourages the ingenuity of manufacturers.

Clause A of the Regulations requires all authorized plumbers to fill up, and leave at the engineer's office, printed forms as notices that new or altered pipes or fittings are ready for inspection, and such notices are required to be given before pipes or fittings are covered.

Notwithstanding the satisfactory working of this system, by which it is made the interest of the plumber to do his work well, it must be allowed that in connection with old fittings difficulties frequently arise, for the removal of which an Act of Parliament should be obtained.

CONCLUDING REMARKS.

The Author has endeavored to show the results of his experience in connection with a system for preventing waste, which has hitherto produced practical results in Liverpool only.

It has been necessary in many instances to refer to special districts, in order to explain particular points, and such districts have been selected as represent the results fairly, so that the remarks upon them are applicable generally to all similar districts.

In low-class districts of the metropolis the waste of water is probably even greater on the average than in Liverpool; but if constant service were restored to any single district, that very fact would in a great measure create its own cure. Many of the outlets for water, which under a few hours' supply are left continuously open, would, under constant supply, become such intolerable nuisances that the flow must inevitably be checked; and if the owners of property became aware that all cases of continuous waste would certainly be discovered by the night inspectors, and watched by the authorities until the cause was removed, in the manner which they might prescribe, the district would soon show a lower consumption than under intermittent supply.

In a system the success of which depends so much upon the careful and politic manner in which the details are attended to, it is obviously necessary that the control of such details should be delegated only to those whose energy and general qualifications render them peculiarly fitted to lay aside the ordinary practice in such matters, and to enter vigorously upon the new processes.

NOTE ON PROF. MERRIMAN'S CALCULATION OF STRAINS IN THE CONTINUOUS GIRDER.

By JOHN D. CREHORE.

Written for Van Nostrand's Engineering Magazine.

Incidental to other calculations which I am making for strains in the continuous girder, I have applied my method to Professor Merriman's example of five unequal spans, given in this Magazine for September, 1876. In his solution for the third span there is one point (probably an oversight) to which I would call attention.

On the 193d page he defines W as "being equal to wl, if w is the load per linear unit;" and with this understanding he writes $A=\frac{1}{4}Wl$. Upon the 198th page he puts $W_1=6\times 8=48$, for six apex loads, $W_2=9\times 8=72$, for nine apex loads, etc.; and "considers the live loads applied at the panel-apices, as uniformly distributed over the spans."

Now, is it correct to consider the sum of these apex loads on any span, as uniformly distributed in the sense in which W has been defined? If it is correct, then in this example we have:

Ton per linear foot.

$$\text{1st span, } \frac{W_1}{l_1}=w_1=\frac{48}{70}=0.6857$$

$$\text{2d span, } \frac{W_2}{l_2}=w_2=\frac{72}{100}=0.7500$$

$$\text{3d span, } \frac{W_3}{l_3}=w_3=\frac{56}{80}=0.7000$$

Tons per linear foot.

$$\text{4th span, } \frac{W_4}{l_4}=w_4=\frac{88}{120}=0.7333$$

$$\text{5th span, } \frac{W_5}{l_5}=w_5=\frac{64}{90}=0.7111$$

whereas there are, actually, eight tons to every ten feet, except at the end half-panels of each span.

The formula, $A=\frac{1}{4}Wl$ is strictly applicable to apex loads, only when the apices are infinitely near each other; in which case we should have, if $w=0.8$,

$$\frac{W}{l}=w\times\frac{l}{n}\times\frac{n-1}{l}=w=0.8,$$

when n, the number of panels, is infinite.

The two formulae,

$$A=Pl(2k-3k^2+k^3),$$
$$B=Pl(k-k^3),$$

which the Professor has used for the apex loads on the third span, are equally required in determining the pier-moments of that span, due to the apex loads on the 1st, 2d, 4th and 5th spans.

For full loads these two formulae give identical results, and the tedious summation of numbers can be avoided by first summing the series.

Thus, for any span covered with the

live load alone (if this were possible) we should have:

$$A=B=Pl^2\Sigma(2k-3k^2+k^3)=Pl^2\Sigma(k-k^3)$$
$$=\tfrac{1}{2}l^2-20\,l=\tfrac{1}{4}\,Wl^2-20\,l,$$

where $W=nP=0.8\,l$,

$l=10\,n$,

$k=\frac{1}{n}, \frac{2}{n}, \frac{3}{n}, \ldots \frac{n-1}{n}$,

For clearness, I tabulate the values of A and B, resulting from their different conditions of loading:

	$\frac{1}{4}$ W l^2 0.8 ton per lin. ft. continuously distributed uniformly.	$\frac{\frac{1}{4}\,W\,l^2}{0.8\,(n-1)}$ / n ton per lin. ft. continuously distributed uniformly.	$Pl^2\Sigma(k-k^3)$ 8 tons at each lower apex, or joint.
$A_1=B_1$	68,600	58,800	67,200
$A_2=B_2$	200,000	180,000	198,000
$A_3=B_3$	102,400	89,600	100,800
$A_4=B_4$	345,600	316,800	343,200
$A_5=B_5$	145,800	129,600	144,000

Now it is manifest that the third column of numbers alone, answers the conditions of the example we are considering; and for the third span our author has used 100,800, instead of 89,600; but for the four other spans he has drawn from the second column instead of the third.

Computing the moments in the third span by using the third set of values of A and B, just given for spans fully loaded, and the values of A and B, which Mr. Merriman has given for the seven separate apex loads on the span, we have the following table:

(*See Table on following page.*)

When this table of moments and strains is compared with that on the 201st page of the September Magazine, the value of my criticism will be sufficiently apparent.

I have called attention to this subject not so much because Professor Merriman, in the case of four spans, inadvertently distributed his apex loads uniformly from pier to pier, as because I have perceived in the practice of some engineers (?), a tendency to treat the apex loads as uniformly distributed continuously, *in order to find the strains as small as possible*, and thus underbid (if the doctrine be accepted) competitors who do not employ that device.

It will be noticed that I derive each maximum diagonal strain, not from its vertical, but from its horizontal component, furnished directly by the sum of the positive or negative differences of simultaneous moments at adjacent lower joints. This obviates the use of "shears" altogether, in the present example.

Thus, for the third pair of braces, the sum of the positive differences of simultaneous moments at the 3d and 4th joints, is

$$\Sigma\,(\triangle M)+=303.3.$$

Then 30.33 is twice the horizontal component of the brace-strain sought; and the strain is

$$30.33\times\frac{2.236}{2}=33.9.$$

Simultaneously $\Sigma\,(\triangle M)-=-76.3$; and the strain due this negative difference is -8.5.

In summing these differences, the difference due to dead load, must, of course, be always present with its proper sign.

The maxima strains in the bottom chord, I also deduce directly from the moments and strains already found, and thus avoid computing and arranging another table.

In the girder under consideration, it is plain that any panel-length or segment of the bottom chord, will sustain its maximum strain simultaneously with the maximum strain in either one or both of the segments directly above it in the upper chord.

If the strains in two adjacent segments of the upper chord, are both maxima for the same conditions of load, then the maximum strain in the segment of the bottom chord, directly below, will be a mean between the two upper strains. Thus for the third segment of the bottom chord,

$$H_3'-=-\frac{41.2+38.6}{2}=-39.9.$$

$$H_3'+=-\frac{-23.5-43.6}{2}=+33.6.$$

after changing the signs.

If the strains in two adjacent segments

		M_3	10	20	30	40	50	60	70	M_4
1	6 P	−63.1	−53.5	−43.9	−34.3	−24.7	−15.0	−5.4	4.2	13.8
2	9 P	446.4	378.4	310.4	242.4	174.4	106.4	38.4	−29.6	−97.7
3	P_1	29.1	−43.2	−35.4	−27.7	−20.0	−12.3	−4.6	3.2	10.9
4	P_2	45.3	−17.5	−80.3	−63.1	−45.9	−28.7	−11.5	5.7	22.9
5	P_3	50.8	− 1.3	−53.4	−105.5	−77.6	−49.7	−21.8	6.1	34.0
6	P_4	47.9	7.2	−33.6	−74.3	−115.0	−75.8	−36.5	2.8	42.0
7	P_5	39.1	9.8	−19.5	−48.8	−78.1	−107.4	−56.7	− 5.9	44.8
8	P_6	26.6	8.3	−10.0	−28.3	−46.6	−65.0	−83.3	−21.6	40.1
9	P_7	12.8	4.4	− 3.9	−12.3	−20.7	−29.0	−37.4	−45.7	25.9
10	11 P	−171.3	−61.4	48.5	158.4	268.3	378.1	488.0	597.9	707.8
11	8 P	28.8	10.3	− 8.1	−26.6	−45.0	−63.5	−81.9	−100.3	−118.8
12	Live Load	726.7	418.4	358.9	406.8	442.6	484.5	526.4	619.8	942.2
13		−234.4	−176.9	−288.2	−420.9	−473.6	−446.3	−339.0	−203.2	−216.4
14	Sums...	492.3	241.5	70.7	−20.2	−31.0	38.2	187.4	416.6	725.8
15	Dead l'd	369.2	181.1	53.0	−15.1	−23.2	28.7	140.5	312.4	544.3
16	Max. +	1095.9	599.5	411.9	385.6	419.4	513.2	666.9	932.2	1486.5
17	Max. −	..	..	−235.2	−436.0	−496.8	−417.6	−198.5	..	..
					TOP	CHORD.				
18	H +	109.6	59.9	41.2	38.6	41.9	51.3	66.7	93.2	148.7
19	H −	..	..	−23.5	−43.6	−49.7	−41.8	−19.8	..	..
					DIAG	ONALS.				
20	$\Sigma(\Delta M)+$	558.4	426.1	303.3	191.3	90.5	1.2	..	..	..
21	$\Sigma(\Delta M)-$	..	..	−76.3	−164.2	−263.5	−374.2	−495.9	−627.5	..
22	Odd brace	−62.4	−47.6	−33.9	−21.4	−10.1	−0.1	..	..	..
23		..	..	8.5	18.4	29.5	41.8	55.4	70.2	..
24	Even brace	62.4	47.6	33.9	21.4	10.1	0.1	..	..	..
25		..	..	−8.5	−18.4	−29.5	−41.8	−55.4	−70.2	..
					BOTTOM	CHORD.				
26	H'+	..	..	33.6	46.6	45.7	30.8	..	..	..
27	H'−	−81.7	−45.5	−39.9	−40.3	−46.6	−59.0	−77.4	−117.3	..

of the top chord become not maxima simultaneously, take the maximum value of each, and find from the table the simultaneous value of the other; and in either case take the half sum. The greater half sum will be the lower segment strain sought, after changing its sign. Thus when

M_{10}=max.=599.5, M_{20}=288.2;

therefore

H_2=59.9 and H_3=28.8. $\frac{1}{2}$ sum=44.4.

When

M_{20}=max.=411.9, M_{10}=498.1;

therefore,

H_3=41.2 and H_2=49.8, $\frac{1}{2}$ sum=45.5;

whence

H_2'=strain in lower chord=−45.5.

THE WELDING OF IRON.

From "English Mechanic."

THE following abstract of a paper by Mr. G. Newcombe, the secretary of the Cleveland Iron Trade Foremen's Association, will be found a valuable addition to the literature of a subject which has recently attracted much attention. Mr. Newcombe calls attention to the ambiguity of language in which the conditions for an effective weld are often stated, and introduces extracts from some of the latest utterances on the subject. Mr. Edward Williams, at the conclusion of a

paper "On the Manufacture of Rails," read before the Iron and Steel Institute in September, 1869, said: "Welding I hold to be the one thing needful, and we should never lose sight of it. As I have before said, the chance of obtaining thorough welding would be much increased by not insisting on more toughness and fibre than is absolutely necessary to guard against so much brittleness as would bring about breakages of the rail in work."

In the discussion on that paper, Sir William Armstrong said that "in the manufacture of guns on the coil system, a perfect welding is just of as much importance as it is in the manufacture of rails. The conclusion arrived at, both at Elswick and Woolwich, was this, that in proportion as the iron has a steely character, so in proportion is it unfavorable for welding. The indication of its steely character was obtained in this way: We took a specimen of iron heated to a certain point and then plunged it into water. If we found its tensile strength was increased beyond a certain limit it was rejected as unfavorable for welding. The iron welds most perfectly which undergoes no increase of strength in the process of hardening."

Wr. Williams, in reply to questions, said, "Mr. Fothergill has asked me what welding is. I believe good welding to be a combination of effects, an actual amalgamation of the surfaces, and soldering together by means of the cinder. In proportion as we have more of the absolute contact and less of the soldering, so is welding good, and *vice versa*. Where there is no contact of the actual metallic surfaces, and nothing but the soldering of the layers together by means of the cinder, we have poor welding; and it is the poorer the thicker the cinder. Where you have a large proportion of surface actually brought in contact with the layer next to it, then you have what we call good welding. But, so far as I know, perfect welding—that is, complete contact of surfaces, or anything at all approaching it—is impossible."

I will now give a quotation from another source. Mr. Mathieu Williams, in his fifth Cantor lecture, in treating on lamination and blistering, says: "When a blacksmith makes a weld in a common open fire, he throws sand on the surface to be joined, the object being to flux the scale—that is, to convert the oxide into fusible silicate. This being done, he brings the fluxed surfaces together, and by hammering forces out the liquid silicate, and thus brings clean surfaces of pure iron together, which at a proper heat unite perfectly. If he had a film of oxide between the surfaces it would prevent welding. Following up this principle, I obtained from the potteries some "slip," or finely-ground flint used in glazing earthenware, mixed this with sufficient water to form a sort of paint or whitewash, and with a whitewasher's brush painted it over the surface of the piles on both sides of each layer. I treated several piles of the finest quality of iron in this manner:—They were rolled into boiler-plates, none of which showed any signs of lamination. I believe that by this means lamination may be effectually prevented."

Further, at the meeting of the Iron and Steel Institute, held at Leeds in the autumn of last year, Mr. Richard Howson, in a paper read before that society, "On Welding Iron," said that in order to obtain "complete metallic contact" the skill of the workman had to be exercised—1st, in heating the iron sufficiently; 2d. in protecting the surface from oxidation by means of a flux; 3d. in forming the surfaces in such a way that the flux has a means of escape when the ends are closed up under the hammer.

Having thus given you a *résumé* of the latest theories on the subject of our paper, I propose, with your permission, to examine them from a practical standpoint, and ascertain how far they are supported or contradicted by the most advanced practice of the day; and as Mr. Howson's views are of the most recent date, and may fairly be supposed to include much that had previously been said on the subject, I propose to take up the consideration of his conditions—first, on the necessity for proper heats to secure good welds. I have no doubt we shall all agree on this condition; in point of importance I think it rightly stands first, for I believe if the primary cause of defective welding could be traced, its origin would in a majority of cases be found in bad heating.

There are other causes which prevent

good welding, but I shall treat of them in another place. There is no operation connected with smithing which requires more careful handling, or gives more anxiety to the smith, than the process of welding, for on the successful issue of one weld in the manufacture of a single article may depend either the success or loss of much labor and money. It may, therefore, repay us to examine with care the conditions to be observed in obtaining a good heat. In the first place the fuel must be as free from metallic impurities as possible, especially sulphur, as it readily combines with iron, and with it forms sulphide of iron, which is naturally detrimental to the formation of a good weld; 2d, by a proper construction of the hearth, and arrangement of tuyere, to obtain the requisite chemical combination necessary for a proper combustion of the fuel for heating purposes. This is effected by placing the tuyere about four inches below the level of the hearth for lighter kinds of work, and from six to eight inches for heavy kinds. But even with good fuel and the arrangement of tuyere just spoken of we may obtain two kinds of heat—viz., a carbonaceous or reducing heat, or an oxydizing or destroying heat; the carbonaceous is that which is required by the smith to reduce his iron to a welding condition, to obtain which he must maintain a constant supply of heated fuel between his tuyere and the iron to be heated, and covering it likewise if the whole mass is to be heated. The chemical action which here takes place may be explained thus:—The oxygen of the air, after passing the tuyere, comes in contact with the heated carbon in the fuel; chemical union then takes place; one part of the carbon combines with two parts of oxygen, forming carbonic acid; this, in passing through the heated fuel above it, takes up another part of carbon and forms carbonic oxide, which is composed of two parts carbon and one part oxygen; and so long as this action can be maintained we have a reducing heat suitable for bringing iron into the welding state with the formation of the least amount of oxide on the surfaces of the iron; and if we fail to obtain those conditions, and allow the fuel to become deficient in quantity between the tuyere and the iron when it is in a semi-welding state, then we have a chemical action of a different kind, for the oxygen then being in excess, through a deficiency of carbon, readily combines with the iron, and forms a cinder or oxide of iron. This combination results in great loss of iron, as will be seen in the following example, which shows a portion of a $\frac{3}{8}$ square bar of iron that has been heated to a welding condition and then placed under a blowpipe. A high heat was maintained for nearly two minutes by simply turning the different sides of the bar to the action of the oxygen. The iron in this condition appears in a boiling state, and the oxide formed ran off in small liquid globules.

If the destructive effect of oxygen is so apparent on a small sample of iron, an approximate opinion may be formed of the great loss resulting from its action on large masses. I believe that in proportion as we obtain heats under either of the two conditions just named, so shall we get good or bad welding. If the oxydizing heat has acted on the iron, it leaves a film of cinder which is difficult to remove, and which prevents close metallic contact of the molecules needful to good welding; this is often apparent in examining large forgings, when turned and polished, that have been laid together or built up in slabs or piles. A dark horizontal line may be traced in the forging which indicates the junctions of the slabs that in heating have been allowed to oxydize, perhaps through the furnace being too slow in heating, or through the admission of too much free oxygen. The oxide not being properly expelled while under the hammer, the result is a defective shaft, the weakness of which is soon made apparent if in performing its work it is subject to much torsion; whereas, if the mass had been heated in a full carbonaceous flame to that fine mellow or spongy condition so essential to a complete incorporation of the molecules, and which renders iron as nearly homogeneous as can be obtained under the piling system, no such thing would happen.

We will now examine the second section of the conditions just quoted as necessary to secure good welding—viz., in protecting the surfaces from oxidation by means of a flux. The views advanced in this section of the author's argument

are so utterly at variance with the best practice of modern times, that I feel compelled to join issue with him on this subject, as I am convinced that it is not necessary to use any flux in order to secure a perfect weld—that is, if the iron is comparatively free from carbon, and the proper conditions of heating have been observed. Large masses of scrap are welded up in our forges, and smaller sections of iron in our smithies without any flux. Indeed, I have no doubt in the process, described by the author, of piling and rolling the large armor-plate at Sheffield, and the lucid description of the manner adopted at Low Moor in manufacturing their famous plates and bars, shows that no silica was used as a flux to assist the welding other than that which the iron contained when it left the puddling furnace; yet the author admits that his samples were as nearly homogeneous as it is possible to get them without absolutely melting the iron. That welding may be effectively accomplished without the use of a flux I think there are few workers in iron prepared to dispute; but as fluxes are used in welding, and chiefly by smiths, we will inquire into the cause of their adoption.

The flux chiefly in use is sand; being abundantly found in nature it is consequently cheap. It is composed of silicon and oxygen, and technically known as silica. It readily melts on being applied to hot iron; and it is this property, combined with its cheapness, that accounts for its general use. Why is it used by the smith? Because in joining two pieces of iron together different kinds of splicing or scarfing are adopted; those, or at least the most common in use, are of a pointed character, and they present an unequal thickness of iron to the action of the heat; and as the point of the scarf is farthest into the fire, and through its unequal thickness conducts the heat much quicker than the heel or thick part of the scarf, it consequently arrives in a welding state first, and if the action of the heat was not checked the point would be burnt away before the heel had arrived at a welding state. To prevent this the smith throws on or dips the back of the point into the sand; the sand on coming in contact with the heated iron melts and absorbs so much of the heat of the part to which it is applied, and on melting becomes vitrified. This glassy silicate readily combines with the iron, and forms a covering to the part exposed to the heat, and being of a very refractory nature, it is some time before it is burnt off the iron. It thus protects the iron in its weak or exposed part, while the other or thicker part is absorbing the heat and arriving at the welding condition. It is sometimes used when the iron is on the anvil, but only when such iron is overheated, and will not bear hammering. A little sand thrown on absorbs the heat and restores its cohesive power. The smith, in using sand, is always careful to keep it from the face of the scarf; he knows from experience that the cleaner he keeps the two surfaces to be welded the closer and more perfect will be the weld. This, I consider, is the legitimate use of sand in welding; it is used as a chemical agent to prevent waste of iron, and even in this capacity should be used as sparingly as possible, for its baneful effects are left behind on the forged articles, which, if they have to be either planed or turned, present on their surfaces a series of knotty or flinty points which blunt the tool and are a source of much annoyance to the operator.

I believe that the use of sand is injurious to iron, and though I admit that it may be used as an agent to prevent waste of iron in some particular kinds of scarfing, I contend that it is not essential to sound welding. In support of this assertion, and keeping clear of the examples before me, I will refer to the welding of tires for railways and tubes for boilers; both these articles are continually under inspection for the purpose of detecting flaws or unsoundness, and they are subjected to continual tensile strain and shocks which tend to develop any flaws or unsoundness, yet how few out of the many thousands in use give way at the weld! and they are invariably welded without any flux being used. Numerous other examples might be given of specialities of manufacture where the welding is done without any flux, but I think they would not add any more weight to the argument I have here advanced.

Before closing this subject there is one matter nearly connected with welding

which has not received that careful attention that it demands, and which, I think, the future interests of the district will require. I refer to the selecting of iron suitable for welding properly together; not that there is any difficulty in welding the Cleveland iron, for it is remarkable for the excellence of that quality, yet there are few districts which produce iron that is more laminated. Perhaps this may, in a measure, be the result of the prosperity of the past few years which has prevailed in the iron trade when quantity and not quality was the great desideratum. In welding hard and soft irons together the difficulty is to get a heat suitable to both, as it is difficult to define the exact temperature for iron in the welding state, for it differs so materially according to the different degrees of quality of iron. The amount of heat which a hard pure iron would absorb before arriving at a proper viscous or pasty condition would be sufficient to destroy a soft impure iron by burning. Iron may be welded at different degrees of heat, varying in color from a greasy yellow up to a white heat, and if heated beyond this point it becomes burnt, though not being fusible when in an uncombined state. Heat has great influence on iron in altering its condition. A high heat will change a fibrous to a crystalline iron, whilst a low welding heat will allow it to retain its fibrous character. Irons in a welding state possess great affinity or attraction for each other, and this is manifested in a greater or less degree according to uniformity of quality. If two pieces of iron are laid together in a welding condition they readily stick to each other, and if the surfaces are of moderate extent it requires some force to pull them asunder. A striking proof of this attraction may be seen in any forge, in the piling of very large masses of scrap containing thousands of pieces, which are heated to a welding state, and then brought out of the furnace, and held in suspension by the middle, between the points of a pair of tongs, and though it may weigh from two to three hundredweight, there is no difficulty in transmitting it from the furnace to the anvil, during which time the particles composing the mass are held together by atomic attraction, but some mechanical force is necessary to bring the particles into closer metallic contact. The difficulty is not so great in welding hard and soft irons together as in keeping them together after they are welded. In a sample before me containing five different kinds of iron, of varying degrees of quality, the welds, so far as we can judge from appearance, seem to be perfect, yet if this sample had much work put on it, if it were upset under the steam-hammer, the harder knots would separate from the softer, their structural forms are so different. The fine crystalline form of the Low Moor iron cannot be thoroughly incorporated with the open molecular structure of the common Cleveland. I believe much of the defective welding found in our Cleveland iron is due to this cause; we find on examination a layer of crystal and another of fibre in regular succession throughout the commoner kinds of iron. A low heat is adopted in its manufacture, purposely to retain its fibrous character; the result is the lamination spoken of. This evil is not confined to the manufacture of bars only; the rail trade has suffered from the same cause. I think you will agree in the opinion, supported as it is by the samples before us, that welding properly performed is neither a soldering nor gluing process. Neither of those words is applicable to the process. I think that it is possible to get a near approach to complete metallic contact by welding; but as the conditions are so varied by reason of the different chemical combinations in iron, it is impossible in the present state of metallurgical science to lay down any fixed rules; therefore, the skill and observation of the workman must supply this want, and be constantly directed to those affinities and combinations which are constantly taking place in all metallurgical operations, under fixed though perhaps undefined laws which govern the results, and give good or bad work in proportion to the extent in which they are regarded or neglected.

ENGINEERING PROFESSORSHIP AT GLASGOW UNIVERSITY.—A legacy of £140 per annum has been left by the widow of the late Rev. Dr. Black, for an endowment in connection with the Chair of Engineering and Mechanics.

WEIGHTS AND MEASURES.

From "Iron."

MR. H. W. CHISHOLM, the Warden of the Standards, has just issued his tenth annual report on the proceedings and business of the Standard Weights and Measures Department of the Board of Trade. Thus, it is provided, by statutes passed in 1859 and 1860, that, throughout Great Britain and Ireland, no copies of the standard weights shall be legal unless reverified in the Warden's Department within five years, and no copies of the standard measures unless so reverified within ten years; but a proviso was added authorizing copies of the country standards to be locally reverified within the same periods, by comparison with one of the country sets which shall have been duly reverified in the Standards Department. It appears that, although the law requires gas-measuring standards to be verified in the department, there is no provision for their reverification.

The number of local standards rejected during the year on account of bad workmanship, or as requiring readjustment, was 325 in respect to weights, and 76 in respect to measures—or a total of 401; being fourteen per cent. on the total number (2,780) verified and reverified. From 1869 to 1873, both inclusive, the number rejected range from eleven to twenty per cent., whereas in 1874 the number was thirteen per cent., and in 1875 only nine per cent., now followed by a rise to fourteen per cent.

A list is given of those places in the United Kingdom for which copies of the Imperial standards (exclusive of troy weights) have been officially verified, but without being subsequently reverified within the period prescribed by law. These defaulting places are 105 in respect to weights and 147 in respect to measures, 87 of the latter being in respect to measures of length. It is stated that, under the existing laws, the Standards Department has no control whatever over the local inspectors of weights and measures, and has no power to compel the production of the local standards for reverification. Mr. Chisholm observes: "The department is thus entirely passive in the matter, and the utmost I can do is to record in my annual reports the several places where the law is not duly obeyed." The list is made up to March 31st, and among the defaulters in respect to the reverification of standards of weight we find such places as Weymouth, Gateshead, Stockton, Harwich, Portsmouth, Winchester, Leominster, Deal, Gravesend, Bolton, Preston, Banbury, Oxford, Shrewsbury, Wolverhampton, Guildford, Reigate, Brighton, Chichester, and York—together with the counties of Chester and Wilts.

A series of standard weights and measures has been verified for the Government of Siam during the past year. Mr. Chisholm says it should be noticed that at Siam the standard temperature for weights and measures, is 85° Fah. The Siamese unit of length is the "wa," consisting of eighty English inches, defined by lines upon a solid bar of standard silver. The Siamese unit of weight is the "chang," called by foreigners the "Siamese catty," its legal weight being equal to 2.675 pounds avoirdupois. This standard weight is constructed of platinum iridium. Altogether eight standards were manufactured for the Siamese Government, and submitted to Mr. Chisholm's department for verification. The standards thus made were found to be correct within a fraction which seemed very small. Nevertheless the error was sufficiently large to exceed the limits prescribed by the Standards Department. Consequently the stamp of the department was not affixed to the standards, but official certificates of verification were given, showing the result of the examination or comparison. The standards were engraved with the royal seal of Siam, and with a shield for an inscription to be engraved upon them on their arrival at Siam. We should fear that the further process of engraving would again disturb the fractions of accuracy in respect to the weights, though Siamese science may rest satisfied with the result.

During the past year a complete set of Russian standard weights having been constructed in London for the Govern-

ment of the Czar, a request was made to Mr. Chisholm by General Gloukhoff, the head of the Standards Departments at St. Petersburg, to compare the several units of this complete set, and to give their values in terms of our Imperial standard pound. The standard weights have undergone a scientific revision in Russia, and it appears that there is a slight departure from the customary figures. The Russian pound was stated a few years ago to consist of 6318.5 grains. At another time it was 6316.81 grains. It is now given as 6319.73146 grains, or 40951196 metric grammes. The standards examined by Mr. Chisholm did not differ materially from the normal values they were intended to represent, and they were accordingly certified in the usual official form to General Gloukhoff.

The time came round during the past year for reverifying, under the Standards Act, the standard weight of 62.321 pounds, being the legalised weight of a cubic foot of water, together with its official copies; and also for reverifying the standard cubic bottle, representing the measure of a cubic foot of distilled water weighed in air against the standard 62.321 pound weight; as well as the gas-measuring standards of one cubic foot, five cubic feet, and ten cubic feet, the contents of which are based upon the standard cubic-foot bottle. The standard cubic foot weight, with its three official copies, was last reverified in 1870. One of the official copies was presented to the French Government in 1871. Consequently only two official copies in addition to the standard remained to be reverified last year. These are made of hard gun-metal, and are cylindrical in form with a knob. They have now been reverified by comparison with the same bullion standards that were used for their original verification in 1859, and for their reverifications in 1868 and 1870.

These comparisons have been checked by further comparisons with the avoirdupois reference standards. Compared with the results obtained in 1870, in which year the weights were readjusted, there now appears an increased weight in the standard to the extent of 5010 grains. The two copies have increased in weight by 4241 grains and 2538 grains respectively. This augmentation of weight is considered due to the oxidation of the comparitively large surfaces which are exposed. These weights do not, however, require to be now readjusted, as their small ascertained errors can be allowed for in comparisons made with them. It is curious that the standard weight showed the least error after the readjustment in 1870, but now shows the greatest. Still the greatest discrepancy is so minute as to amount to less than one part in 80,000, the present error of the standard weight being 5,404 grains out of 92,321 lbs. The cubic-foot bottle has increased 4.12 grains, or 0.056 parts of a cubic inch in capacity since 1869. The present error, in excess, is 174.4 grains, or 0.090 part of a cubic inch, being about one part in 2,500. The three standard gas-holders were readjusted and put into complete working order in 1875. Consequently, their errors are but slight, though the cubic foot gas-holder is rather less correct than it was in 1869. The present ascertained errors, in parts of a cubic foot, are a deficiency of 0.0025 in the cubic foot gas-holder, an excess of 0.0030 in the five cubic feet holder, and an excess of 0.0110 in the ten feet holder. The errors allowed to the local gas measuring standards are appreciably greater than the foregoing, so that none of the latter will be rejected through errors in the imperial standards.

It is a singular omission in the Sale of Gas Act that it makes no provision for the periodical reverification of either the official gas-measuring standards, or the copies deposited with the three metropolitan cities, nor, indeed, of the many copies verified for the use of the local inspectors of gas-meters. On receiving a suggestion from Mr. Chisholm, the chief magistrates of London, Edinburg and Dublin at once consented that the gas-measuring standards in their custody should be put into complete order, and the cost paid out of their corporate funds. The process had not been completed at the date of the present report, so that further reference to the subject is postponed. The propriety of this reverification is shown by the fact that the official gas-measuring standards in the keeping of Mr. Chisholm's department were found to require taking to pieces and

setting to rights, so as to rid them of the injurious effects of oxidation.

The question of a standard photometer for measuring the illuminating power of coal-gas comes under discussion in the present report, and would seem to be as important as the institution of a standard of quantity. The instruments now in use for measuring the lighting power of gas include the 100-inch photometer invented by Mr. G. F. Evans; the 60-inch photometer designed in 1863 by Dr. Letheby, and since adopted by the metropolitan gas referees, as well as by the authorities in Canada; Mr. G. Lowe's jet photometer, said to be used as a standard in Italy, and the illuminating power meter introduced by Mr. Sugg. The Evans photometer requires no dark closed room for its operations, and on that account is much used in hot countries. It is also much used in the United Kingdom, particularly by companies supplying gas of high illuminating power. Concerning Sugg's illuminating power meter, it is claimed that the lighting quality of the gas can be determined in one minute from the quantity of gas (measured by an experimental meter which is a portion of the apparatus) found requisite to maintain a flame of gas three inches high for sixty seconds. In Lowe's jet photometer, also, the lighting power of the gas is deduced from the quantity required under specified conditions to maintain a flame three inches high. The photometers of Evans and Letheby are based on Bunsen's method, a disc of paper, partially greased, being interposed between the standard light and the gas flame. The disc is moved to or from one or the other of these lights, until the greased spots are, as near as may be, imperceptible to the eye. It is then tolerably certain that the transmitted light from the one flame is equal to the reflected light from the other. The flame nearest to the disc is therefore the weakest, and the relative distances afford a measure of the power. The method in some degree resembles Count Rumford's equalization of shadows.

Mr. Chisholm observes that "the time has not yet arrived when any photometer hitherto constructed can be relied upon for giving constant accurate results." The difficulty consists in obtaining a trustworthy unit—that is to say, a standard light certain to afford the same degree of illumination so long as the known conditions are unaltered. Hitherto no better standard than the sperm candle has been found. Sperm candles burn at rates varying from about 115 to 126 grains per hour, and the gas examiner has to make allowance for this varying rate. Although a correction is thus applied according to a specified rule, it is doubtful whether the same quantity of sperm always yields the same light, as there is reason to believe that an effect is produced by the quality of the cotton wicks and the method of their plaiting. Mr. Chisholm is informed that a sperm candle recently brought from Russia and tested here against a regulation sperm candle was found to be ten per cent. better in quality, and to have proportionately increased illuminating power.

Attempts have been made to employ Mr. Crookes' radiometer as a photometrical instrument. But it is doubtful whether the action of the radiometer is not rather due to heat than to light. Mr. Chisholm says it is of importance to add that, in the event of any standard photometer being adopted, it is absolutely necessary that authoritative regulations for a uniform mode of using it should be prescribed. This gentlemen further observes that the question of legalizing a standard sixty inch Letheby photometer in this country, as it has been already legalized in Canada, is well worthy of consideration.

An additional set of standard decimal grain weights has been constructed during the year, to be used specially in trials of the pyx. These weights are made of hard brass, and it is proposed to take every precaution in their use, by strictly following the official regulation never to touch standard weights with the hand, but to lift them either with a proper fork, or with a wash-leather, carefully wiping them with a wash-leather after use.

A somewhat notable event of the year has been the laying down at Trafalgar Square of several standards of length embedded in a solid platform of granite. All the details of this operation are given in the appendix to the present report. The platform is situated at the foot of the north wall of the square, and the

entire cost has been about, £450. These mural standards are now available for public use, like those erected outside the wall of the Royal Observatory at Greenwich, only the Trafalgar Square standards are on a much larger scale, the platform being nearly 260 feet long. The Standards Commission, in one of their reports, recommended that mural standards of length should be securely fixed for public use in all populous towns, the expenses connected with them to be defrayed out of the local funds. The Warden of the Standards has accordingly sent a circular letter to the local authorities urging the expediency of their carrying this recommendation into effect; at the same time offering them such information as in his judgment would facilitate their course of action.

Various other matters are referred to in the little volume before us, and it is evident that the department of the Warden of the Standards is not only extremely useful, but of increasing importance. It is impossible not to admire the high degree of scientific skill displayed in the various operations, as well as the zeal with which the duties of the department is discharged.

THE MOVEMENT OF THE SEA, VIEWED IN ITS HYDRAULIC RELATIONS TO PORTS AND SHORES.

By A. CIALDI.

From "Rivista Marittima," translated for Proceedings of Institution of Civil Engineers.

In the autumn of 1873 the Royal Venetian Institute of Science, Letters, and Art offered a prize of 3,000 lire for the best essay on the movements of the waves and littoral currents. The competitors were invited to discuss the most accredited theories, and with observations of recognized phenomena, to deduce a more complete theory, especially with reference to the compound action of waves and currents on the change of coast lines, and on the efficacy of maritime constructions; such as might tend to the improvement and preservation of ports and shores, especially those of Italy. The essays might be written in either Italian, Latin, French, German, or English. Signor Cialdi had, in the "Rivista marittima" for 1874, given his "Preliminary Notions as to a Treatise on the Construction of Ports in the Mediterranean," the second chapter of which, treating of winds, waves, and currents, which he regarded as the most important, has been much controverted in Italy. The invitation of the Venetian Institute induced him to appear as an anonymous competitor; citing his own former works, as well as those of his opponents.

The essay, which was the only one presented, although greatly praised for its elaborate learning, was not thought to fulfill sufficiently the conditions of the Institute, and did not receive the prize. It is divided into three parts, each headed by a distinct paragraph of the programme given by the Venetian Institute. The first part contains two articles,—on the accredited theories of the movement of liquid molecules in the formation or development of marine waves; and on tidal and littoral currents. The former article commences by the definition of certain terms, and then refers to the theory of Newton on the velocity of waves, and to that of Gerstner and the brothers Weber, published in 1825, on the orbital movement of the liquid molecules in waves. Mr. Merrifield's "Summary of the Theory of the Oscillating Sea Waves" is also cited. Signor Cialdi concludes this portion of his work by expressing the opinion that he has demonstrated that the theories hitherto produced, in order to explain the motions of the sea by means of mathematical analysis, are not of much use with reference to the questions of the construction of harbors, or the advance or retreat of the shore.

In the second section Signor Cialdi asserts that the tidal wave attains, in the high seas, a velocity of not less than two

miles per hour; which rises to six, or even ten miles per hour, according to the obstacles which it encounters on the shores. He holds that in the same degree that these obstacles diminish the velocity of the propagation of the wave, they augment its transporting power, and cites experiments made in France and in England, to show that, at a velocity of six inches per second, water transports soft clay; at one of twelve inches per second, it transports ordinary sand; and at that of thirty-eight inches per second, it transports pebbles as large as a hen's egg. The transporting power of the sea-water he takes to be higher than the above, inasmuch as its density is $\frac{1}{30}$th more than that of fresh water. In calm weather the tide wave on the Italian coasts remains limpid. In stormy weather the Author considers that the turbid matter which it contains is removed in the same manner in which it is brought by the tidal current; so that the tides of the Mediterranean have not a sensible action on the shores of Italy.

He then discusses the "Mediterranean current," known as the littoral or "grazing" (*radente*) current, which flows from the right hand to the left of the observer who faces the sea; and which attains in the Adriatic a velocity of three or four miles in the twenty-four hours; and in the rest of the Mediterranean, on the average, a velocity of eight miles in the same space of time. He gives an observation of his own, made from a small steamer anchored near Corneto, in 12½ feet depth of water (on the 4th of August, 1857), of the bottom of the sea, where he saw the marine algæ bending, perpendicularly to the shore, from west to east, showing the direction of the gentle current at the spot. As the tops of these sea-weeds were about 8½ feet below the surface, he concludes that the littoral current was unfelt at that slight depth, and that the littoral current has little or no transporting power. His investigations are limited to the Adriatic, as that sea has been chiefly studied by Montanari and his followers: and holds that the laws which regulate the accretion of shores must be the same on all the coasts of the world. He then discusses the prevailing winds on the Adriatic, and concludes this section with a note to the effect that it is to be regretted that the littoral currents have been rather sought for in maps and books than measured in the sea itself.

The second part of the treatise discusses first the materials which become obstructions on shores, and then what the Author calls the "garland of adventitious deposits." This region, which D'Archiac has called the "zone of sanding-up" (*atterissements*), extends, according to Signor Cialdi, from the coast line to a depth of 328 yards in the ocean, of 164 yards in the Mediterranean, and of 87½ yards in the Adriatic and in the English Channel; he says that the waves of the sea have a visible transporting power to a depth of 218 yards in the ocean, of 54½ yards in the Mediterranean, and of 44 yards in the Adriatic and the Channel. The materials deposited in this zone he regards as derived, first, from the deposits brought down by rivers; secondly, from the erosion of portions of the sea coast; and thirdly, from the productions of the sea itself; that is to say, from the remains of marine plants and animals. Entering at considerable length into recent works on the natural history of marine life, he concludes that the deposits on the shores of the Italian seas are formed, in the Mediterranean, by thirty per cent. from river deposit, twenty per cent. from erosion of shores, and fifty per cent. from organic bodies; and in the Adriatic, by thirty-five per cent. of the first, five per cent. of the second, and sixty per cent. of the third.

The second section of this part of the essay proposes to establish a complete theory as to the compound action of the waves and currents in altering the coast line, and affecting marine works. It speaks of the depth to which the movement of the waves is felt, and of the submarine current which the Author holds that they generate. It mentions the effect of the wind, in raising the waters on the shore above their mean level by about nineteen inches in the Tuscan Sea, 3¼ feet in the Adriatic, and 6½ feet in the ocean. It refers to the report of the International Commission on the Bay of Pelusium, and to the westward current there prevailing, which flows in an opposite direction to the assumed littoral Mediterranean current. It refers to the action of the dominant

winds upon the waves, the phenomena of which he discusses at considerable length.

In the third part the Author treats first of the hydraulic works carried out, especially on the western and southern shores of the Adriatic, the experience derived from which confirms, in Signor Cialdi's opinion, the theory of the wave current. He refers to the deposits of the Po and of the Adige, and to the changes in the *foci*, or openings through which the waters of these rivers finally join the Adriatic, that are due to the variation of the winds.

The second section of this part discusses the subject of such harbors as Signor Cialdi calls port-basins. He concludes that when such a harbor (of which Ancona furnishes an example) has only one mouth or entrance, this must always be placed on the leeward side, or away from the dominant winds. He refers to the actual and the prospective increase in the size of vessels, especially steamers; and to inconveniences in the ports of Naples and of Genoa in consequence of their being open to the prevailing winds.

In the third section Signor Cialdi treats of what he calls canal-ports, which he says are always formed on level, and therefore either sandy or gravelly shores; and are on that account very difficult to maintain. The convenience of navigation demands that such ports should either be at the mouth of a river, at that of a lagoon receiving the tidal water of the sea, or joined by an artificial canal. The essential feature of these ports he takes to be the construction of two parallel jetties, or "guardian moles," leading to the sea. He cites the area of the lagoon of Malamocco (which is 40,200 acres), and the width of the canal between the jetties (which is 515 yards), as an example capable of general application under the second division; the Sulina mouth of the Danube being a case of the first kind. The third case, that of the purely artificial canal, is illustrated by the entrance to Port Said. As to this, Signor Cialdi cites the report of Colonel Stokes, R.E., to the effect that the repeated extension of the pier seems to be a hopeless feature in the method of dealing with the difficulty of the constant deposit of material in the bay; a deposit which is computed by that officer to have amounted to more than 5,000,000 cubic yards within three years.

In the fourth section of this part of the treatise, Signor Cialdi proposes his own method of meeting the difficulty of a canal-port. Instead of two parallel jetties, he proposes to make the leeward jetty much shorter than at present is usual; and to cut the windward jetty into two parts, by an opening about the spot where the bar is generally formed. The jetties should be built with a slightly curved plan, with the convexity opposed to the prevailing wind. At the end of the first part of the longer jetty (where the opening is made) the sea wall is returned to the windward at right angles or thereabouts. It is anticipated by Signor Cialdi that the action of the wave current, deflected by this return, will keep free the opening between the two parts of the main or windward jetty, and thus maintain a free entrance to the harbor.

The work concludes with a summary of the arguments adduced to show that the theory of the wave current is that which explains the action of the sea on the shore, and is alone applicable to the improvement of ports or the preservation of coast lines.

THE MEASUREMENT OF FORCE.*

By ROBERT MOORE, C. E.

Written for Van Nostrand's Engineering Magazine.

In the January number of the Eclectic is an article by Prof. De Volson Wood, of the Stevens Institute of Technology, on the measurement of force, in which several of the positions taken are erroneous, and the whole article as coming from a teacher and author of a treatise on mechanics so remarkable that it ought not to pass without notice.

The main purpose of the article, though

* This paper was read before the Engineers Club of St. Louis.

not very easy to discover, seems to be to show that the absolute unit of force mentioned by Prof. Tait, in a late lecture before the British Association is not as good a measure of force as the pound. In holding thus, he takes issue not with Prof. Tait alone, but with nearly every modern writer on mechanics.

The chief reason assigned for this position is that the pound measures pressures, as well as motions, which he seems to think is not true of any other unit or mode of measurement.

And in particular he says that forces are in general not properly measured by momentum, which is a measure of constant forces only.

In order to show the amount of error involved in these several propositions as well as to bring into clear view the truth concerning the subject itself, it will be best to state as briefly as possible the main doctrines concerning the measurement of force.

Force is a word by which we designate the unknown cause of motion. It is defined as "That which moves or tends to move a body or to change its rate of motion." Of force in itself aside from its effects, we know nothing. In measuring force all we can do is to measure the effect and from this infer the magnitude of the cause. The measurement of force is, therefore, resolved into the measurement of the motions of material bodies, and to do this we must take into account and measure separately, two things:—first, the body itself, and second its velocity or rate of motion.

To measure the body we assume some particular body or piece of matter as a unit, and then compare this unit with the body in question.

The unit is wholly arbitrary and may be anything whatever, a piece of metal, or a volume of water, or some other kind of matter. The unit in most common use in England and America is a piece of platinum called the "Standard pound." The comparison of a body to be measured with the unit may be made in two ways, the first is by the method of volumes. If the substance to be measured is of the same kind as the unit their masses will be as their volumes. This is the way in which we measure liquids, earthwork, grains, &c.

But if the substance to be measured is of a different kind from the unit, we make use of the record method—the method of weighing.

In this method we compare the masses of bodies by means of the effect produced upon them by the force of gravity. If in a scale a body is balanced by one unit we call its mass one: if it takes two units we call its mass two and so on. In doing this we take as our major premiss the assumption which all experienc verifies that at any given place the force which gravity exerts upon bodies is in direct proportion to the quantity of matter which they contain.

This action of gravity upon bodies is their weight. Weight is not matter but a force acting upon matter. But owing to the fact that the force and the body have usually the same name they are often confounded.

Thus, we speak of a pound or kilogramme of matter, and of a pound or kilogramme of force, but the two meanings should be kept carefully distanced. Weight is used to measure matter just as volume is, but matter weight and volume are wholly distinct and unlike.

The weight of a body varies with the intensity of gravity, its gravity of matter is invariable and may be the only invariable thing about it.

To measure the velocity of the body we need two units—a unit of space and a unit of time.

For the measure of velocity is the amount of space passed over in a given time, the velocity varying directly as the space and inversly as the time.

So if we let v and v' be two velocities, s and s' the spaces described, and t and t' the times employed we get:

$$\frac{v}{v'} = \frac{\frac{s}{t}}{\frac{s'}{t'}}$$

If now in this equation we let v', s' and t' each equal unity; that is, if we take as our unit of velocity a velocity in which a unit of space is described in a unit of time, we shall have,

$$v = \frac{s}{t},$$

by which, of course, we do not mean that there is any likeness or identity between velocity, space and time, nor that we

actually divide space by time. We mean only that the number of units of velocity is the same as the number of units of space divided by the number of units of time. The equality is one of number only. For uniform motions the foregoing is all we need. The measure of one record is there the measure of every other. But if the velocity be changing, we can only measure it at each instant.

This we do by passing to the elementary values of s and t, assuming the motion to be uniform for the space ds and the time dt. As the preceding formula is true for all values of s and t, it must be true for all values of s and t, it must be true for ds and dt, so that we have

$$v=\frac{ds}{dt},$$

which is an expression for the rate of motion at the given instant dt.

In variable motions, however, we need to know more than the rate at a given instant.

To define the motion completely, we need to know the rate at which it is changing, which rate is called its acceleration. But the acceleration manifestly varies directly as the amount of the change and inversely as the time. That is

$$f=\frac{v-v_0}{t},$$

in which f is the acceleration, v_0 the velocity at the beginning, and v the velocity at the end of the time t. When we pass to the elementary values $v-v_0$ becomes dv and t becomes dt. We then have:

$$f=\frac{dv}{dt},$$

which gives us a measure of the acceleration when the rate of change is not constant.

With these measures of mass and motion, both variable and constant, we are now ready to measure the motions of bodies and pass from them to a comparison or measurement of the forces by which they are produced.

In making the passage from motion to force we are obliged to assume several postulates which are the result of experience and lie at the basis of all dynamics. These may be stated thus:

1. All motion, or change of motion, either in magnitude or direction in material bodies is the result of force. This is Newton's first law of force and may be called the principle of inertia. It enables us to infer the presence of force wherever we see motion begin or change. It does not, however, give us the amount of force. It is qualitation, but not quantitation. We, therefore, need to go further and assume,

2. That forces acting upon equal masses during equal times, are to each other as the changes of velocity which they produce.

3. That the time and total change of velocity being the same the forces are to each other as the masses in which the given change is produced.

4. That with a given mass and a given change of velocity, the forces are to each other inversely as the times during which they act. Or, in other words, the less the time required to produce a given effect the greater the force, and vice versa.

By putting these postulates into algebraic symbols we get the following equation in which is contained the whole doctrine of the measurement of force,

$$\frac{F}{F'}=\frac{\frac{M(v-v_0)}{t}}{\frac{M'(v'-v_0')}{t'}}$$

in which F and F′ are two forces. M and M′ the masses upon which they act, $(v'-v_0')$ and $(v-v_0)$ the changes of velocity, and t and t' the times in which these changes are produced. Now, if in this equation we make F′, M′, $v'-v_0'$, and t' each equal to unity, we get:

$$F=\frac{M(v-v_0)}{t},$$

which is the simple and more common form of the fundamental equation, and is also the one from which are derived all the equations made use of by Prof. Wood in the article in question.

But by the supposition upon which his equation is based we have made F′, the unit of force to be a force, which acting upon the unit of mass during the unit of time, will change the velocity by unity.

This is the absolute dynamic unit adopted by Prof. Tait, and by nearly all modern writers on Mechanics. It is in-

deed the corner-stone of the whole science. Its exact value is determined by our units of space, mass and time, and by these alone.

In England and America, there are the foot, pound and second, so that the British absolute unit of force is a force which acting upon a mass of one pound during one second will produce a change of velocity of one foot per second.

It is about the force which gravity exerts upon a mass of half an ounce.

Prof. Wood, however, objects to this measure of force as not being sufficiently general, and claims that the proper unit is the pound, meaning, of course, the pound of force, or the action of gravity upon the unit of mass. To determine the exact nature and value of this unit, let us go back for a moment to our last equation,

$$F=M\frac{v-v_0}{t}.$$

As we have just seen, when speaking of acceleration, the factor,

$$\frac{v-v_0}{t}=f \text{ or } \frac{dv}{dt}=f,$$

as the case may be, so that the preceding equation may be written

$$F=M\frac{dv}{dt}=Mf.$$

This last is the form used by Prof. Wood, and the study of which he very strongly and very properly recommends as a means of clearing up any confusion which may exist on the subject.

Let us now apply this equation to the action of gravity which as we know produces at the surface of the earth upon all bodies upon which it acts unhindered, an acceleration of about 32.2 feet per second which is represented by the letter g. Putting G for the total force and taking the mass, M, as unity, we get,

$$G=1 \text{ lb. force}=Mg=g=32.2 \text{ absolute units.}$$

From which we see that gravity acts upon the unit of mass with a force of about 32.2 British absolute units of force.

Prof. Wood also considers the case but with a very singular result. He says :—"If in the expression $F=Mf$, we make M and f each unity and consider f as constant, we have, 1 lb. of force=1 lb. of mass $\times$ 1 foot velocity. The second member of which is the unit proposed by English writers for measuring force."

Now concerning this equation, in which the word velocity probably means acceleration, we can only say that whilst it might be true in some other planet where the acceleration of gravity was only one foot per second, it is certainly not true in this one where the acceleration is over 32 feet, unless, indeed, Prof. Wood, has some private meaning for the word pound which he has forgotten to divulge. Taking the words in their ordinary meanings the equation is simply untrue, and we are at a loss to see how it could ever have been written. We should be disposed to credit the printer with the blunder and not the Professor did not the argument seem to depend upon it. As it is we must wait for an explanation until we hear from Prof. Wood himself.

Resuming the equation $G=Mg$, in which, where M is one pound of matter, $G=g$ is one pound of force, we see that the force pound is of the same nature as the absolute unit, and measurable by it. Both are forces, and each may be used indifferently as a measure of force. They differ only in the fact that one is g times as great as the other; so that if we have the measure of any force in absolute units, and wish to get its measure in pounds, or gravitation units, we have only to divide the former by g to get the number sought. Thus, let $F=Mf$ be the measure of a force in absolute units, and F_0 be its measure in pounds. Then will

$$F_0=\frac{F}{g}=\frac{Mf}{g},$$

and *vice versa*, $F_0=F=Mf$, or the measure of any force in pounds, multiplied by the acceleration of gravity, gives the nature of the same force in absolute units. But Prof. Wood argues that the pound has a superiority over the absolute unit, in this way, viz.: That whilst the absolute unit "is only applicable to moving bodies," the pound measures also pressures and tensions in which there is no motion, and hence he says. "is the more natural, and certainly the more general measure of force." Now, in taking this position, the Professor

seems to go upon the assumption that there is some radical difference between forces which produce motion and pressures or tension which do not produce motion, so that the mode of measurement applicable to the one may not be applicable to the other. Otherwise his argument has no force at all.

But this distinction is wholly superficial and not based upon any difference in the things themselves. A pressure, or a tension, is simply a case of equilibrium in which two forces balance each other, so that the result is not motion but rest. But let one of the forces be taken away, and the result is motion, and the magnitude of the force is measured in the manner just set forth, viz.: by measuring the quantity of motion which it produces when acting freely. For example: a body supported by the hand produces pressure, in which the reaction of the hand holds in check the action of gravity upon the body. Now, let the hand be taken away, and the motion which follows in the body is the exact measure of the pressure, which it exerted the moment before. So that the method of measuring force by means of motion is sufficient for forces of all kinds whether they actually produce motion or not.

Moreover, if this were not true the pound would still have no advantage over the absolute unit. For, as we have just seen, the pound and the absolute unit are exactly the same in kind. Both are forces, and there cannot possibly be anything measurable in terms of the one which is not equally measurable in terms of the other. A pressure can be measured just as well in absolute units as in pounds. For some reasons the pound is the more convenient unit. It is more generally known, and does not involve as high numbers as the other. But aside from its convenience it has no superiority whatever.

On the other hand the pound has one feature which makes it for scientific purposes much inferior to the absolute unit, viz., its lack of certainty.

The pound of force, or the action of gravity upon the unit of mass is not a fixed quantity, but varies with the latitude and with the altitude of the place. But the absolute unit depends for its value only upon the units of mass, space, and time, each of which when once chosen is subject to no variation. The absolute unit is therefore the same at all times and in all places.

A force upon the sun or one of the planets measured in this unit, would have exactly the same measure anywhere in the universe. It is therefore, the only scientific unit, Prof. Wood to the contrary notwithstanding.

Prof. Wood has also a discussion upon the subject of momentum as a measure of force, in which he is no more fortunate than in his treatment of the subject of dynamic units. "Momentum," he says "(Mv) is measured by foot pounds and hence generally is not a measure of force." Now even if momentum were measured in foot pounds as the Prof. says, the validity of the conclusion which he draws from it is by no means evident. Force must be measured by its effects, and it is certainly conceivable that we should take its effect in foot lbs. as our measure, even though we may have some other and better measure.

The horse power, which is a force capable of doing 33,000 ft. lbs. of work per minute, or 550 ft. lbs. per second, and which is the commercial measure of force, is in fact of exactly this kind. The truth is that force may be measured by any one of its measurable effects.

But momentum is not measured in foot pounds. The foot pound in the universally accepted sense of the term, is the overcoming through the space of one foot of a constant resistance of one pound, and is a measure of work but not of momentum, which is a wholly different thing.

For momentum is the numerical product of mass by velocity, whilst work is the product of force by space, and involves neither the idea of mass nor of velocity.

Momentum is, therefore, wholly different in kind from work, and has nothing to do with foot pounds. This difference is seen very clearly in the algebraic symbols of the two, which are:

$$\text{Work} = Fs = \text{Force} \times \text{Space}.$$

$$\text{Momentum} = Mv = \text{Mass} \times \text{Velocity}.$$

In which we see that neither quantity involves a single element of the other.

Prof. Wood seems to have fallen into the common error, already referred to,

of confusing the mass pound with the force pound, and to have confused momentum and work.

But in order to see how momentum is used in measuring force, let us go back again to our fundamental equation,

$$F=M\frac{v-v}{t}.$$

If in this the initial velocity $v_0=o$, we get

$$F=\frac{Mv}{t},$$

which means that for this case at least force is measured by momentum, divided by the time in which it is generated. But this equation, as Prof. Wood very correctly remarks, pre-supposes that the force F has a constant intensity during the whole of the time t, otherwise the value given whilst it may be the mean value of the force, may not be its actual value at any given instant. And on this fact he bases the conclusion, which he underscores, that momentum is a measure of constant force only.

But this conclusion does not by any means follow. It is, indeed, true of all measurement that the thing measured must remain constant during the measurement. Otherwise we cannot give it any definite or certain value whatever and measurement becomes impossible. This is true of variable, as well as constant quantities. So that when the quantity to be measured is variable we are obliged in order to meet this condition to take an interval of time indefinitely small during which the quantity is regarded as a constant and measured as such. Thus, in the case before us, if the intensity of the force be changing we take the indefinitely small interval dt during which we suppose the force to be constant. For this interval $v-v_0$ becomes dv and we get the expression already found

$$F=M\frac{dv}{dt}.$$

But in both cases our method is exactly the same differing only in the length of time considered, and both formulae may be embraced in one general statement to the effect—that in all cases the measure of the force is the momentum which it causes, divided by the time in which it is produced. So that momentum may be used as a general measure of force whether it be variable or constant. In the course of his discussion of this question, Prof. Wood gives a short argument which, as illustrating the looseness of his reasoning, it is perhaps worth while to quote in full.

Speaking of constant forces he says: "For this case we have $Ft=Mv$. Similarly for another force acting during the same time we have

$F't=M'v'$. Hence $F : F' :: Mv' : M'v'$,

And by assuming one of the forces as a standard, or unit for measuring all other forces, we have:

$$F=\text{Unity}=M'v',$$

Therefore $F=Mv=M'v'$."

But he has just said that $F't=M'v'$ and F' can equal $M'v'$ only upon the condition that $t=1$, a condition which he does not give.

In the general case which he is considering, F=Unity is not equal to $M'v'$, and his conclusion should be

$$F=\frac{Mv}{M'v'},$$

a result utterly without value. The conclusion which he actually, though erroneously reaches, but which is true for the special case in which the time also is unity, viz:

$$F=Mv$$

is somewhat important. For it is the basis of the general statement by Prof. Clerk Maxwell (*Theory of Heat*, p. 83), that "a force acting on a body is measured by the momentum it produces in its own direction in unit of time."

The same principle is also laid down, though in slightly different words by Prof. Magnus (*Mechanics*, p. 62), in a book which is edited and endowed by Prof. Wood himself. It is true that on such a subject authorities are as a general rule of no value. If our equations do not carry their own evidence, it is of no use to appeal to great names.

But in our present discussion of momentum as a measure of force, there is one other authority which is certainly pertinent and which we cannot refrain from citing. It is that of Prof. Wood's own book just published, on "The Elements of Analytical Mechanics." On

page 51 of this book, after finding the general equation applicable to variable forces

$$F=M\frac{dv}{dt},$$

he goes on, and in italics, of which he is quite fond, lays down this principle: "Hence the momentum impressed each instant is a measure of the moving force."

So that on the authority of Prof. Wood himself, we may, in spite of the article under consideration, take it as proven that momentum is a general measure of force.

STEAM INJECTORS.

Translated from the French of M. LEON POCHET.

II.

MEANS OF AUGMENTING THE PERFORMANCE OF THE INJECTOR PUMP.—WATER INJECTORS.

We should consider two reservoirs of water R and R′ (Fig. 3) placed at the heights, respectively H and h, above a reservoir S. R serves to raise the water from S, and discharge it into R′. Let us place below the reservoir R a vertical tube C, terminating in a nozzle A, entering into a funnel-shaped opening of a second vertical tube D, leading to the reservoir R′. If we open the stopcock of the tube C, the water will run out by the nozzle A with great velocity, carrying a part of the surrounding liquid, and if the apparatus is well regulated, it will be able to raise the water to the height of the tube D, and it would flow into the reservoir R′. The fluid vein proceeding from the upper reservoir R will, by communicating its motion to the water of the reservoir, carry it on to R′. We will consider the condition of the working of the apparatus.

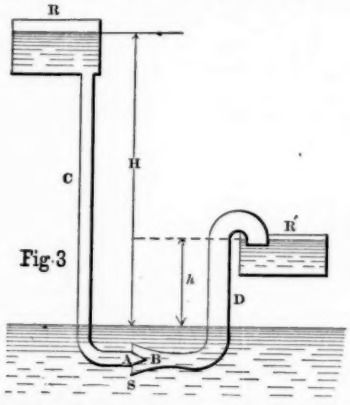

Fig. 3

Let P be the weight of water delivered at the nozzle A;

P′ the weight of water carried per second;

v the velocity of the water leaving the nozzle;

u the velocity of the water at its entrance into the tube B.

Suppose the velocity of the affluent water about the tube B to be neglected, the interchange of quantities of motion will give us the equation

$$Pv=(P+P')u \quad . \quad . \quad . \quad (I)$$

Suppose that the water falls into the reservoir R′ without appreciable velocity, then all loss of living force will be avoided, consequently the velocity u will be determined by the condition

$$\frac{u^2}{2g}=h,$$

whence

$$u=\sqrt{2gh} \quad . \quad . \quad . \quad . \quad (J)$$

but

$$v=\sqrt{2gH} \quad . \quad . \quad . \quad . \quad (K)$$

To be sure we neglect the friction. Substitute these values of u and v in equation I, and we have

$$P\sqrt{2gH}=(P+P')\sqrt{2gh},$$

whence,

$$\frac{P'}{P}=\frac{\sqrt{H}-\sqrt{h}}{\sqrt{h}} \quad . \quad . \quad . \quad (L)$$

To ascertain the modulus of such an

engine, it will be necessary to divide the useful work

$$W_u = P'h,$$

by the motor work or propelling force,

$$W_m = P(H-h).$$

This will give:

$$\rho = \frac{P'}{P}\cdot\frac{h}{H-h} = \frac{\sqrt{H}-\sqrt{h}}{\sqrt{h}}\cdot\frac{h}{H-h} = \frac{\sqrt{h}}{\sqrt{H}+\sqrt{h}}$$

or again,

$$\rho = \frac{1}{1+\sqrt{\frac{H}{h}}} \quad . \quad . \quad . \quad (M)$$

The elevation H is necessarily higher than that of h. The minimum of ratio $\frac{H}{h}$ is 1, hence the useful mechanical work will always be less than $\frac{1}{2}$.

We have supposed that the conveyed water comes to the nozzle A with no velocity. This is not so; and to make it more easily understood, we will take a very large funnel MN (Fig. 4).

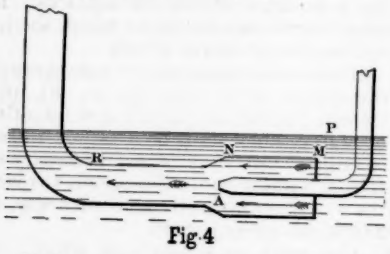

Fig. 4

If the suction of the tube at M differs but little from that at R, it is clear that the velocity of the water carried will be quite sensible.

Call this velocity v'. The equation (I) should be written in the following manner:

$$Pv + P'v' = (P+P')u \quad . \quad . \quad (N)$$

Since the apparatus is placed nearly at the surface of the water in the discharging reservoirs, the flow of water between the two points M and N can be produced only by difference of pressure. At M we have atmospheric pressure augmented by the slight height of water MP. At N we should have atmospheric pressure diminished by a small amount. If we call x this difference or *depression* expressed in head of water, we shall have

$$v'^2 = 2gx \quad . \quad . \quad . \quad . \quad (O)$$

The velocity v of discharge at the nozzle A will be given by the equation

$$v^2 = 2g(h+x) \quad . \quad . \quad . \quad (P)$$

and the common velocity of the water mixed in the tube R should satisfy the relation

$$u^2 = 2g\,(h+x) \quad . \quad . \quad . \quad (Q)$$

In short, if we call ω, Ω, O, the sections of the pipe and of the tube at M and at R, we shall have the relations:

$$\left.\begin{aligned} P &= 1000\,\omega\, v \\ P' &= 1000\,\Omega\, v' \\ P+P' &= 1000\,O\,u \end{aligned}\right\} \quad . \quad . \quad . \quad (R)$$

which gives the following:

$$O\,u = \omega\, v + \Omega\, v'.$$

The heights H, h and the weight P being given, the equations (N), (O), (P), (Q), (R), will allow us to calculate seven unknown quantities

$$P', \quad v, \quad v', \quad u, \quad \omega, \quad \Omega, \quad O, \quad x.$$

Here are eight unknown quantities. One of these is to be determined before solving the problem. It is the value x of the difference of pressure, mentioned above, and which varies with the adjustment of the apparatus.

The formula expressing the work indicates in what way this value varies.

The useful work is

$$W_u = P'h,$$

and the motor work or propelling force

$$W_m = P(H-h).$$

The rendering of the system is then

$$\rho = \frac{W_u}{W_m} = \frac{P'}{P}\,\frac{h}{H-h}.$$

Now, from equations (N), (O), (P), (Q), we deduce:

$$\frac{P'}{P} = \frac{v-u}{u-v'} = \frac{\sqrt{H+x}-\sqrt{h+x}}{\sqrt{h+x}-\sqrt{x}} \quad . \quad . \quad . \quad (S)$$

Substitute in the value of ρ, it becomes:

$$\rho = \frac{\sqrt{H+x}-\sqrt{h+x}}{\sqrt{h+x}-\sqrt{x}} \cdot \frac{h}{H-h} = \frac{\sqrt{h+x}+\sqrt{x}}{\sqrt{H+x}+\sqrt{h+x}},$$

or again,

$$\rho = \frac{1+\sqrt{\frac{x}{h+x}}}{1+\sqrt{\frac{H+x}{h+x}}} \quad . \quad . \quad . \quad (T)$$

If we compare this formula with (M), we discover that they are the same, when we make $x=0$. This supposes the velocity $v'=0$. It is easy to prove that the performance (T) increases with the difference x, and that it is always less than 1. We shall have the theoretical limit of its value in making $x=10^m,33$. This difference may be artificially made by raising the injector above the reservoir (Fig. 5), which will force the water to rise to the height C D.

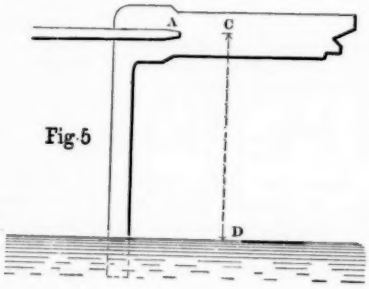

Fig. 5

Suppose

$$P=1, \quad H=500^m, \quad h=5^m.$$

If, at first, we suppose the depression to be nothing, the equations (L) and (M) give us:

$$P' = \frac{\sqrt{500}-\sqrt{5}}{\sqrt{5}} = 9,$$

$$\rho = \frac{1}{1+\sqrt{100}} = 0,091.$$

To arrange the apparatus in a way to realize a depression of five meters, we make $x=\rho$ in equations (S), (T), and we have:

$$P' = \frac{\sqrt{505}-\sqrt{10}}{\sqrt{10}-\sqrt{5}} = 20,85,$$

$$\rho = \frac{1+\sqrt{\frac{5}{10}}}{1+\sqrt{\frac{505}{10}}} = 0,211.$$

By this hypothesis the performance will have more than doubled. The arrangement of the apparatus has then a great influence upon its action. It should be so that the water raised arrives with considerable velocity at the injector. For this object we direct currents of water by means of several successive funnels (Fig. 6).

Fig. 6

This arrangement is found in several machines of English origin.

We readily understand that the jet of a steam injector may be used as a water injector.

A steam injector conveys, for example, fifteen kilogrammes of water per kilogramme of steam discharged, and communicates a velocity capable of surmounting a pressure of five atmospheres. In other words, the height to which the jet will be able to rise is $41^m,32$.

If we make this liquid jet pass through the nozzle A in Figs. (3), (4), (5), (6), we will be able to carry a new quantity of water, which, it is true, is not raised so high. But if it is not necessary to raise the water above five meters to reach the $41,^m32$, it is clearly to our interest to adopt this arrangement.

The same considerations which we have recommended to place the injector above the supply reservoir are applicable here.

We will now give an account of the theoretical performance of the apparatus in (Fig. 7).

The proportion of water carried by the steam jet is reckoned from the height at which the injector is placed above the discharging reservoir, from the pressure of the boiler and from the amount of water practically raised. Equations (D) and (E) will furnish the proportion of water carried and the velocity of the mixture. The reservoirs B and C being at the same pressure, that is to say, the

atmospheric pressure diminished by the height A R, the equation (B) will give:

$$v=\frac{w}{1+y}.$$

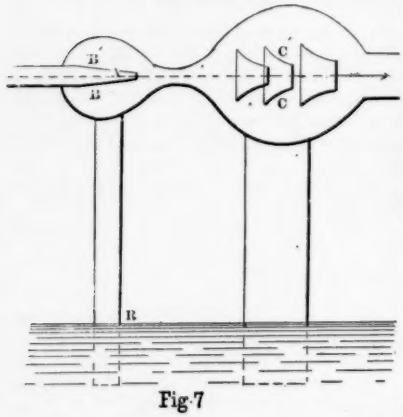

Fig. 7

Knowing the velocity v of the liquid jet at its entrance into the chamber C, we calculate the weight of water carried by the jet by means of formula (S), in giving a start to the depression x, and in calculating the dimensions of the suction apparatus by means of equation (R). We will remember in equation (O) that the velocity v' represents the projection upon the axis of the jet, of the real velocity of the raised liquid streams. It will be necessary, then, to take into account the angle at which the funnels taper.

It is practice alone which can determine the best arrangement of this apparatus, whose theoretical principles only we have established.

We will show by an example how these theoretical calculations may be made.

Example.—To raise the water from the hold of a vessel, knowing the height of suction to be five meters; and the fall of the discharge, five meters. The pressure of the boiler is three atmospheres.

The table (3) gives us for the velocity of the flow of dry steam of three atmospheres at $\frac{1}{2}$ atmosphere final pressure, 739 meters. It will be $w=739$. We will then have equation (D):

$$v=\frac{w}{1+y}.$$

If we make $y=20$, we will find:

$$v=\frac{739}{21}=35^{m},20.$$

The height designated by H is here:

$$\frac{v^2}{2g}=H=63^{m},18.$$

Since the pressure which exists in chamber C about the funnels, is only $\frac{1}{2}$ atmosphere, we will be able to realize at the axis of the jet only a slight depression; we will suppose two meters. It will be, then, $x=2$, and we will have equation (S):

$$\frac{P'}{P}=\frac{\sqrt{63,18+2}-\sqrt{5+2}}{\sqrt{5+2}-\sqrt{2}}=4,41.$$

The weight of water positively raised then will have been:

$$4,41\times 20=88^{k},20,$$

per kilogramme of discharged steam. The mechanical work produced has for its value:

$$88^{k},20\times 10^{m}=882 \text{ kilogrammeters},$$

per kilogramme of steam.

The table (1), that for a weight of water raised to $89^{k},63$, which is nearly equal to 88,20; the Giffard injector produces only 197 kilogrammeters. If we keep the injector at five meters above the surface of the water to be raised, and make use of this apparatus without the intervening injector of water, we should calculate the weight of water carried in the following manner. We should have for the necessary velocity to cause the jet to attain to five meters of height of discharge:

$$v=\sqrt{2g\times(5+5)}=14 \text{ meters},$$

$$1+y=\frac{739}{14}=52,80,$$

whence

$$y=51,80.$$

The weight of water varied would be only $51^{k},80$, in place of $88^{k},20$, which we have found in employing the water injector.

The steam injector arranged with a water injector to serve as a pump, has then a notable advantage over one used solely for steam. Several machines founded on these principles are used in England for draining mines. The pre-

Velocity of Flow of Dry Steam under Different Initial and Final Pressures.

The velocity of a mixture of steam and water, containing x of steam and $1-x$ of water, may be obtained by multiplying the numbers of the table below by $\sqrt{x}$.

Pressures and Final Temperatures.		Pressures and Initial Temperatures.							
		8 at. 170°,81	7 at. 165°,54	6 at. 159°,22	5 at. 152°,22	4 at. 144°,00	3 at. 133°,91	2 at. 120°,60	1 1/2 at. 111°,74
Pressures.	Temperature.	485,79	489,71	494,11	499,13	545,02	512.22	521,70	527,99
atmos.	degrees.	meters.	meters.	meters.	meters.	meters.	meters.	meters.	meters.
7	165,34	223	..	..	..	..	..	..	..
6	159,22	325	238	..	..	..	..	..	..
5	152,22	411	349	258	..	..	..	..	..
4	144,00	494	445	381	283	..	..	..	..
3	133,91	580	540	490	421	349	..	..	..
2,4	126,46	636	601	558	501	420	279	..	..
2	120,60	676	645	606	555	485	372	..	..
1,8	117,30	698	668	632	584	519	417	190	..
1,6	113,69	721	694	658	613	553	460	276	..
1,4	109,68	746	719	686	645	588	504	347	..
1,2	105,17	773	748	717	678	625	549	463	274
1,00	100,00	803	779	750	714	666	596	477	366
0,90	97,08	819	797	769	734	688	621	498	409
0,80	93,88	837	815	788	755	710	647	543	451
0,70	90,32	856	835	809	777	735	676	578	494
0,60	86,32	877	857	833	802	763	706	615	539
0,50	81,71	900	882	858	830	792	739	655	585
0,40	76,25	928	910	888	861	826	770	699	636
0,30	69,49	960	944	924	899	866	821	751	694
0,20	60,45	1,002	988	969	947	918	877	814	765
0,10	43,21	1,065	1,052	1,037	1,017	993	958	906	865

ceding considerations demonstrate that the employment of these machines is not very convenient. Their use is justified only when it is necessary to accomplish rapid drainage with simple apparatus.

EJECTOR CONDENSERS.

Let us conceive that we put an injector on the escape pipe of a condensing-engine. The apparatus will work as usual, that is to say, according as the escaped steam arrives it becomes condensed by the contact with the cold water furnished by the reservoir, will maintain a vacuum, and the mixture of water with the condensed steam imparting a great velocity will be capable of surmounting the excess of exterior atmospheric pressure over the pressure of escaping steam.

We shall thus be able, by the simple interposition of the injector apparatus, to supplant the air pump and all the accessories, and we shall economise the work absorbed by this pump, often very considerable.

Such is the principle of ejector condensers, the employment of which tends, probably, to become general.

Professor Rankine has reported the experiments made in 1868, upon a condenser ejector of the Morton system. The apparatus (Fig. 8) differs from ordinary steam-injectors in that the cold water is drawn by the central tube; the escape of steam is distributed about the central jet by the very long and concentric funnels. In this way the living force of the cold water suffers no loss.

This living force is considerable since the presssure which exists about the cold water, at the moment in which it mixes with the jet of steam, is necessarily less than the pressure at the escape, without which the steam would not flow out, so that the water possesses the ve-

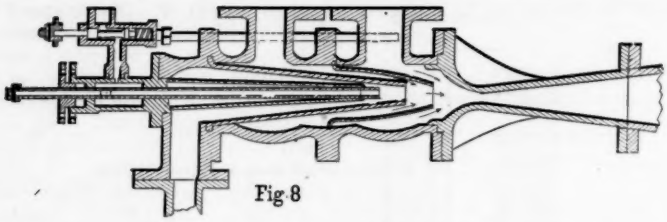

Fig 8

locity due to the excess of the atmospheric pressure over the pressure of the escape.

In the above named experiments Prof. Rankine has found the following results:

	Per sq. centimeter.
Absolute pressure at the boiler.	3k,427
Absolute pressure at commencement of escape.....	0k,756
Mean pressure maintained behind the pistons by the condenser ejector.....	0k,299
Pressure kept near the funnels.	0k,210
	Centigrade.
Temperature of cold water....	8°,4
Temperature of water of condensation.....	30°,3
Weight of cold water employed per kilogramme of steam....	28k,40

These results are, as we see, very satisfactory. They are not widely different from those which were obtained by the air-pumps, but these latter require a notable expenditure of moving force.

In the machine experimented, Rankine valued the effective force at twenty-four horses, and the economy realized by the replacing one horse power air-pump at four per cent.

The theory of the condenser ejector does not differ from that of ordinary injectors, only there has been no account taken of the velocity at which the cold water arrives, that here has considerable value, which is neglected in our general equation (C). To take account of it, we should add to the first member of this equation a term

$$\frac{AU^2}{2g}y,$$

representing the living force of the weight of water y.

In reality we will be able to neglect the term $\frac{V^2}{2g}$.

The determination of the velocity and the calculation of the dimensions of the apparatus are made, according to the method explained in beginning of article for April. This is a problem which presents no difficulties.

The Morton apparatus has one peculiarity which we ought to describe. To put the apparatus in motion, we allow a priming of steam from the boiler to pass through a central tube. It may happen that the pressure falls below the proper limits, for the working of the apparatus. Under these circumstances, the cold water flows into the escape-pipes, and then into the cylinders. Every time that this inconvenience threatens the central steam jet is automatically opened by a spring piston, and its power communicates to the cold water jet a sufficient impulse to prevent its deviation towards the cylinders, and re-establishes the normal working of the machine.

THE INJECTOR EMPLOYED IN A HYDRAULIC PRESS.

Suppose that we place an injector at the foot of a cylinder of a hydraulic press (Fig. 9). The jet of hot water may be introduced into the cylinder so that the pressure will be lower than that which corresponds to the velocity of flow. We should be able then to work a hydraulic press with a pressure

$$\frac{V^2}{2g}.$$

Suppose the pressure at the boiler be five atmospheres, the jet according to table, page 213, could rise to the height of 482 meters; this corresponds to

482,000 kilogrammeters per sq. meter,

or to

50 atmospheres.

The same table shows that the mechanical work realized in these conditions is only

3063 kilogrammeters,

per kilogramme of steam used.

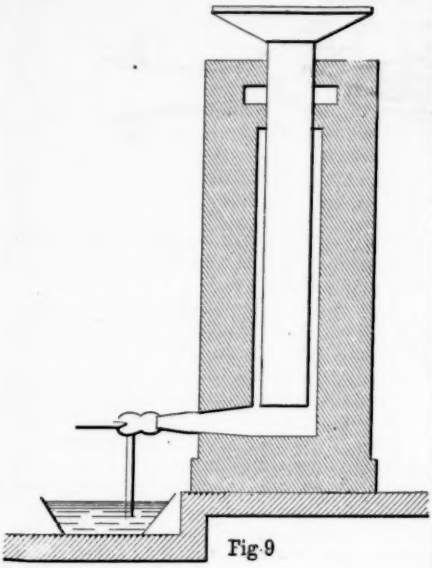

Fig. 9

The work diminishes in the same proportion as the pressure diminishes. Consequently, it would be better to work with high pressure and so diminish the diameter of the hydraulic press.

This is a novel application of the injector, and may prove of service in situations where a hydraulic press is necessary, and where a pump of sufficient power is wanting.

Such an application has not yet been made, at least to our knowledge, and needs preliminary experiments.

PUMPING GAS BY STEAM.—EXPLANATION OF THE FEEBLE WORKING OF FEEDING OR DRAINING INJECTORS.

That which causes the weakness of the performance of a steam injector employed as a draining pump, is the disproportion between the height to which a liquid mixture may be raised, which is several hundred meters, and the height to which we in reality raise them. The apparatus does not give its maximum of performance excepting for very great heights. The disproportion which exists between the specific gravity of the body raised, which is here water, and that which is carried along with it, that is to say, steam, is another source of loss.

In effect, the relative velocity of the water raised at the moment when it mixes with the condensed steam, is so slight that it is not worth taking into account. Let us suppose, on the contrary, that the steam escape in a gaseous medium which it will drag along by a sort of lateral friction (see Fig. 10), and as in a water injector. It may happen that the velocity of affluent air be considerable, consequently cannot be neglected.

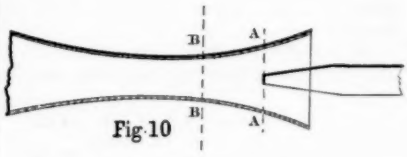

Fig. 10

Now this circumstance will augment the performance, just as in the water injector.

Let W be the velocity of the steam, w that of affluent air, v that of the supposed mixture in the contracted section B B, y the weight of air drawn per kilogramme of steam.

The momentum of the steam will be

$$\frac{W}{g},$$

that of affluent air,

$$\frac{w}{g}y,$$

that of the mixture,

$$(1+y)V.$$

Neglecting the difference of pressure in the sections A and B, we will have the following equation:

$$W+wy=(1+y)V,$$

whence we find

$$V=\frac{W+wy}{1+y}.$$

The living force of the steam was

$$\frac{W^2}{2g},$$

that of affluent air,

$$\frac{w^2}{2g}y,$$

that of air after the mixture,

$$\frac{V^2}{2g}y.$$

The performance of the apparatus will be then:

$$\rho = \frac{y\frac{V^2}{2g}}{\frac{W^2}{2g}} = \frac{y\left(1+\frac{w}{W}y\right)^2}{(1+y)^2}$$

When we suppose the velocity of the material drawn to be nothing, as that in the Giffard injector for feeding boilers, $w=0$, and the formula of the performance is reduced to

$$\rho' = \frac{y}{(1+y)^2}.$$

We see at once how feeble this action is when y is not very small.

When $y=1$ $\quad \rho'=\frac{1}{4}=0{,}25$
When $y=15$ $\quad \rho=0{,}058.$

If, on the contrary, the ratio $\frac{w}{W}$ be slightly increased, the term $\left(1+\frac{w}{W}y\right)^2$ will increase very sensibly the value of the mechanical performance.

For example.—If we make $y=15$ and $\frac{w}{W}=\frac{1}{6}$ the ratio realisable in practice, the calculations give $\rho=0{,}72$ in place of 0,058, which we have found in making $w=0$.

This simple discovery suffices to make us understand that steam injectors are more likely to give good results when employed as gas pumps than when used as water pumps.

LOCOMOTIVE EXHAUST.

The exhaust of a locomotive is only an application of preceding considerations. Only in these machines the steam jet is intermittent, notably augmenting the results which we should obtain with a continuous jet. People do not know that the draught of the locomotive hearth is, if I may say so, due only to the escaping steam; this is, truly, the fundamental principle of the construction of these powerful machines.

Prof. Zeuner has demonstrated by calculation, and by experiment, that the weight of air drawn into the chimney of a locomotive is proportional to the weight of steam expended. So the combustion is more active when the engine works fastest.

It has been long known, in a general way, that the velocity of a locomotive engine ought to be pushed to its utmost limits when required to perform important work, as when ascending a slope.

Generally the proportion of the weight of air drawn to the weight of steam employed, is between 2 and 3 to 1.

M. Péclet reports the experiments made by M. Glépin upon the draught produced by the continuous steam jets opening into cylindrical tubes. The results are quite various, according to the diameter and the length of the tubes. We have usually found that the performance increases with the length of the tubes. Its value has been a maximum with the tubes from $0^m{,}50$ to $0^m{,}55$ in diameter, and from 3 meters to $3^m{,}50$ in height. It was then raised to 0.1145. We here call performance the proportion of the living force of air drawn to the living force of the steam jet.

It seems demonstrated that the intermittent jets produce superior action, and according to M. M. Flachat, and Petiet the work produced by the intermittent injections of steam in the chimneys of locomotives varies from 0.5 to 0.16 of the work which the steam is able to produce.

Usually in this kind of apparatus it is necessary to multiply the surfaces of the contact of steam and air. Then annular jets are better adapted for such work than the compact cylindrical jets.

STEAM BLOWERS.

Several important processes are founded upon the conveying of air by a jet of steam. This method has been applied to the ventilation of mines. It was also employed to ventilate the great machine gallery at the Paris Exposition, in 1867.

To conclude, M. Siemens has made a new application in the manufacture of steel. In his apparatus, the air is drawn by a double tube through an annular jet and a central conical jet. The air is introduced by an annular central orifice, and by orifices through the outside partition of the apparatus. The contacts of the fluid entering and the fluid escaping are thus greatly multiplied, and this circum-

stance is eminently favorable to the action.

The mixture of steam and air is introduced into a reservoir containing bits of coke crushed and washed by a current of cold water. The steam is condensed and the air escapes mostly free from steam.

As a means of forcing currents of air for purposes of ventilation this method of M. Siemens is worthy of consideration.

RAILWAY CURVES AND HIGH SPEEDS.

From "The Engineer."

MUCH as has been done in the improvement of permanent way as laid on most of the railways in this country, there cannot be any doubt that the high speeds and great weight of the rolling stock now used on these lines necessitate a more perfect system of way than has yet been adopted. It is true that while new the tracks used are strong enough to carry their loads, but sufficient marginal strength is not given to these to allow of their being capable of continuing to do this except under the most scrupulously efficient inspection and maintenance, and even with this there are weaknesses which gradually increase, but which are not always apparent. The rails are in almost all cases of ample strength, but more efficient means are needed by which the gauge may be rigidly maintained. This remark is especially true as regards those lines where curves are frequent and where trains run at high speeds over them. One of the most valuable deductions derived by Baron von Weber from his experiments on the stability of the permanent way of railways, the results of which he published in 1869, was that the ordinary forms of way owed their stability and powers of resistance to the enormous strains visited upon them by fast heavy trains, in a very large degree to the insistent weight of those trains; and that were it not for the tie formed by the axles of the engine and vehicles, through the friction of their wheels on the rails, the resistance of the latter afforded by the spikes holding them to the sleepers would be wholly insufficient to prevent their motion laterally, or their canting to such an extent that engines and vehicles would be continually dropping between the rails and causing accidents which, as it is, are not very frequent, not, however, by reason of the inherent strength of the permanent way, but by virtue of the tie which every axle forms. Weber also took care to pronounce a warning against relying upon this strength only imparted for the moment to the track by the train running over it, because that borrowed help is always an uncertain and variable element in the resisting power of the line. All engines, and especially six-wheeled engines, vibrate on the central wheels, so that the vertical oscillation at the leading and trailing wheels is in many cases very considerable. This oscillation is in effect equal to alternately increasing and decreasing the loads on these wheels, and therefore, in the latter of these cases, to take away all help to the permanent ways afforded by the tie which the axle of each loaded wheel forms. This help, then, is taken away just when the rails need most support—viz., when the engine is plunging and wandering.

Baron von Weber found, in a series of experiments extending over a considerable time on engines constantly running, that the load on the springs mentioned was sometimes so increased as to exceed the normal load by over a hundred per cent.; while, on the other hand, the load was sometimes so reduced as to be only about seven per cent. of the normal load. This shows how important it is that the track should in itself be of sufficient strength without relying in the slightest degree upon any such help as may be afforded by the weight of the engine and train. Every railway engineer has seen evidence of the tendency of the track at each end of a curve to move outwards, the result of

the tangential direction which the engine and train tend to pursue, but which is prevented by the rails, not, however, without the gradual but limited movement outwards referred to. It is at these places that a combined pitching and wandering motion of the engine is greatly increased, if not originated, and the motion generally continues with more or less severity throughout the curve, for the engine, in attempting to pursue a right line direction, is continually being thrown from the outer to the inner side of the curve and back again by its right line tendency and centrifugal force. On curves, therefore, and at the approach to these, the permanent way should be especially strong and capable of rigidly preserving its gauge. The experiments of Baron von Weber were carried out on permanent way laid with flat-footed rails fastened down by hook-headed spikes. This is one of the worst forms of way, though much used abroad; and although the results Weber obtained as to the resistance of the way to spreading would have been very different if made on our chair-supported rails, his results give very valuable and applicable information.

Many of the accidents which follow derailment take place at or near curves, and it seems questionable whether the old practice of giving to the outer rail the amount of super-elevation which theory dictated should not be returned to. It is of course true that this super-elevation cannot be made to suit different speeds or curves; that which would be necessary for fast trains would be much too great for slow trains. But the effect of too high an elevation for the latter would not be so serious as an insufficient amount for the former. High speeds are moreover the rule now rather than the exception, so that for the larger number of trains the full super-elevation is necessary. At present the most general practice is to give the outer rail a super-elevation of only from 1 in. to 2 in., but, on a curve of, say, 20 chains radius the centrifugal force would, at a speed of forty-five miles per hour, be rather more than one-tenth of the weight in motion, and the necessary super-elevation would therefore, on a 4 ft. 8½ in. guage, be rather more than 5⅝ in., or roughly, about three times as much as is usually given. With an engine run ning perfectly steadily and following exactly the curve, the lateral pressure tending to cant the outer rail, would with the above amount of super-elevation, be practically nothing; but with a long heavy train the case would be different, as the engine would exert some, and the hinder wheels a considerable amount of lateral pressure, due to the tendency of the train to pull straight. There is also another condition, and an important one, which tends to increase the lateral and vertical pressure on the outer rail and take the load off the inner rail.

This is the fact, that the centrifugal force acts in the center of gravity of the engine or vehicles, and therefore tends to lift these off the inner and tilt them on the outer wheels, and this and its effect will of course be greater as the center of gravity is higher. Slight variations in resistance and speed will therefore cause the weight to be alternately lifted off and dropped on to the inner rail, oscillations and wandering will be set up especially with the engine, and the forces visited upon the outer rail will be alternately much greater and smaller than normally, and the amount of the forces quite uncertain and unascertainable.

When, however, only a very small super-elevation of the outer rail is adopted, as is usually now the practice, these forces become enormously increased, especially as regards those due to the horizontal oscillations. With an engine weighing 40 tons, and running at the above speed on the assumed curve of 20 chains radius, the centrifugal force supposing little or no super-elevation of the outer rail, would be about 4 tons while the engine ran quite steadily round the curve. The last condition, however, seldom obtains, for the increase and decrease of the load on the opposite springs, due partly to the lifting and dropping above described, gradually set up both a pitching and wandering motion, and as the engine is urged from the inner to the outer rail the latter receives an impact strain very feebly represented by the 4 tons due to centrifugal force. The tendency to spreading of the gauge will be most intense on approaching or entering a curve; and although it would

perhaps be impossible to arrive by calculation at what it would be at these points, it is evident that it would be much more nearly represented by the energy of the engine than by that of centrifugal force. Although it may not be necessary to look for any radically new system of permanent way, it certainly is of the greatest importance that, considering the heavy engines and loads and the high speeds now so largely observed, the track, if of the present forms, should be of the strongest description, laid and maintained in the most careful and thoroughly efficient manner possible. Consideration of the question here referred to, seems to us to suggest that the high speeds now adopted for the express trains on some of the South London lines from Ludgate-hill and Victoria, can only be observed at great risk, even though the road be maintained in good order. When it is remembered that some miles of these lines are carried on viaducts of some height and through thickly populated districts, it will be easily imagined that the derailment of one of these express trains, loaded as they often are to the full, at or near one of the many curves, would be attended by the most appalling results ever chronicled. The recent accident to the Pullman car train on the Midland Railway, and that to the Falmouth express, were both on curves, and were probably largely due to the spreading of the gauge by the strains visited on the outer rail by the forces referred to.

THE FREIGHT COMPETITION OF 1876.

From the Eighth Annual Report of the Railroad Commissioners of Massachusetts.

In the last annual report of this Board, a somewhat detailed account was given of the severe competition among the through east and west lines which had existed during a large portion of the year 1875, under the influence of which rates were reduced to a point lower than had ever before been known. In December of that year, at the time the report in question was prepared, a combination among the through lines had been at last effected, and it was understood that the war of rates was to cease. The difficulty had arisen between the Grand Trunk line and its eastern connections, to and from competing points in New England on the one side, and the Boston and Albany and New York Central on the other. It thus, as did not require to be pointed out, affected the interests of Boston more immediately than those of any other city in the country, though the struggle involved the whole question of through rates. The settlement usually in such cases was finally effected,—the more direct line agreeing to a division of through business with the less direct, based upon a rule of apportiontment which was supposed to secure to each a fair share of the business;—in this case the through business was divided on the basis of the amount of it done by each line during the two previous years. In other words, the business was practically "pooled," a fixed schedule of rates was agreed upon, and competition ceased. At the time, the members of this Board expressed the opinion that this arrangement in no way touched the root of the difficulty, and that it would prove to be merely temporary. This speedily proved to be the case.

The combination of December, 1875, was, in fact, of even shorter duration than any of its numerous predecessors, for it lasted scarcely one month. On the 7th of February, it was broken in consequence of a misunderstanding between the Erie and New York Central railroads, and a new war of rates was begun on all east-bound through freights, under the influence of which they fell rapidly. This continued until the 2d of March, when another meeting of the representatives of through lines was held, and renewed efforts were made to bring about a combination. These, however, resulted in nothing, except a brief postponement of an inevitable struggle. They wholly failed to touch the real root of the difficulty. This no longer lay in the old and chronic inability of

the railroad officials to put any trust in each other's good faith, and rigidly to enforce a scrupulous regard to agreements upon their subordinates. The struggle had assumed a new and, to the railroad interests, far more dangerous form, that of a bitter rivalry between the great commercial cities of the seaboard. Baltimore and Philadelphia were not only asserting an ability to compete with New York City as exporting points for Western produce, but owing to the thorough organization and perfect development of their great through railroad lines, they were demonstrating their power to do it. Ever since the opening of the Erie Canal in 1825, a monopoly of the business of exporting produce has been practically conceded to New York. As is very well known, until within the last ten years it was not supposed that railroads could compete for the carriage of cheap and bulky articles with lake or even slack-water navigation. Rates, however, have generally fallen, until it has at last been demonstrated that under certain favorable conditions it is more advantageous at all seasons to forward nearly every description of merchandise by rail than by water. Accordingly, the amount of agricultural products moved by rail from west to east, as compared with that moved by water, has gradually risen until at the close of navigation (December 2) of the year just ended, it amounted to more than half of the whole quantity moved. In 1873, the proportion was 29.8 per cent. moved by rail to 70.2 per cent. by water; in 1874, it was 33 per cent. by rail to 67 per cent. by water; in 1875, it was 41 per cent. by rail to 59 per cent. by water; and at last, in 1876, it was 52.6 per cent. by rail to 47.4 per cent. by water. This transfer, also, had taken place notwithstanding the fact that during the years named the pressure of competition had forced down rates on wheat carried by lake and canal from Chicago to New York by more than one-half,—from 19.2 cents per bushel to 9.5. Lower than this they could not go, and at this rate the railroads were taking the traffic. Under these circumstances, it was inevitable that a wholly new phase of competition must be developed. Canal navigation was possible to New York alone; but when the traffic passed from the canal to the railroads, other cities possessed equal if not superior advantages. Accordingly, the struggle was no longer between the railroads leading to New York and the Erie Canal, but between railroads leading to different seaboard points. The monopoly of New York was threatened. Neither was the result of the impending struggle by any means so certain, as long habit might induce many people to suppose. The prescriptive enjoyment of an undisputed monopoly has produced in the case of New York City the usual results, and both railroads and business community of that place, confidently relying on long possession and natural advantages, had allowed abuses to creep in, or failed to supply improved facilities, until the handling of produce for export there was made most unnecessarily expensive. Meanwhile, the cities of Philadelphia and Baltimore, having great natural disadvantages to overcome, were naturally forced to husband every resource and make the most of every circumstance in their favor. All this they did with a degree of sagacity, foresight and success, well deserving the careful study of other and more favorably located communities. To Massachusetts, and the city of Boston especially, their experience is very suggestive. The policy pursued by Massachusetts and Boston during the last twenty years of great railroad development, has been in fact the direct opposite of that pursued by Pennsylvania and Philadelphia, or by Maryland and Baltimore. Not only also has it been the direct opposite, but it still continues to be so. In the case of the two last-named communities, the fundamental principle of their through railroad development has been a complete and thorough concentration of force,—the idea of local competition in through business received no favor. It seemed to be instinctively appreciated that the struggle was not between rival lines leading to Baltimore or to Philadelphia, but between single thoroughly developed lines leading to those cities and other lines leading to New York. Accordingly, the whole resources of the two communities were, under the direction of very able men, devoted to the complete development of these single lines. Meanwhile, in Mas-

sachusetts, during the early period, the Boston & Albany, then known as the Western railroad, had been in great degree built up by the aid of the State, exactly as the Pennsylvania and the Baltimore & Ohio had been. Had the same policy of concentration been subsequently pursued, it would have led to the complete and thorough development of that line, without any regard to local competition, and to the securing by it at the proper time of the connecting roads necessary to give to Massachusetts and to Boston an independent all-rail route to the West. This would have been perfectly feasible, down to a time as late even as the year 1868. The New York Central and the Lake Shore & Michigan Southern lines might have been made just as much a part of the Boston & Albany road as the Pittsburg, Fort Wayne & Chicago is a part of the Pennsylvania road, or its Chicago extension is a part of the Baltimore & Ohio. Most unfortunately, a theory of railroad development, natural, perhaps, and reasonable enough in the early and experimental days of the system, but long since abandoned elsewhere, still held possession of the public mind of Massachusetts; and, indeed, seems even yet to retain its influence over it. This community wholly failed to realize that a final struggle was to be between concentrated lines to rival cities, and rested in the firm conviction that it would always continue to be one between rival lines to the same city. Accordingly, while New York, Philadelphia and Baltimore were stretching out to all the great centers of the West, the business vision of Boston seemed limited to the mouth of the Erie Canal at Albany, or at the farthest to the eastern extremity of Lake Ontario. Instead, therefore, of concentrating the hopes and resources of the community on the complete development of one great through line, the public attention was dissipated and the public funds were sunk in such hazardous enterprises as the Hoosac Tunnel and the Boston, Hartford & Erie railroad. Now that the railroad system has more fully developed itself, the unfortunate consequences of this mistaken policy have become at once obvious and irreparable. That policy, however, is still clung to, though rather it would seem from a popular inability to adopt any other and more positive line of action, than from any particular faith in it. The "toll-gate system," so called, as applied to the Hoosac Tunnel line, is the last illustration of a theory of railroad development now utterly abandoned outside of the limits of Massachusetts,—the theory that a community in the struggle for through business with other communities will derive more benefit from a weak competition between a number of undeveloped and incomplete railroad lines, than from the action of a single powerful and concentrated one.

Meanwhile, it was the natural outcome of the other policy in the changed relative position of Baltimore and Philadelphia towards New York which, during the early months of 1876, was gradually driving the great lines into a fiercer and more destructive war of rates than had ever been known before. New York City, and consequently the main railroad line leading to it, began for the first time to realize that its easy supremacy no longer existed, and that in the struggle of competition it had no advantages to waste. Theretofore it had always been the practice on shipments from Western points to the seaboard to take into consideration the distances of the several cities from the point of starting. A concession had always been allowed in favor of the southern points of shipment, under which originally the rate to Boston had been five cents per hundred more than to New York, that to New York five cents more than to Philadelphia, and that to Philadelphia five cents more than to Baltimore. These differences had subsequently been modified until, for some time previous to March, 1875, on all export merchandise, rates to Boston and New York were equal, while those to Philadelphia and Baltimore, though equal to each other, were five cents less than the New York-Boston rate. As the sense of pressure from the competition of the more southern through fares increased, however, the New York interests began to realize that this arbitrary rate placed them under a too heavy disadvantage. Accordingly, a new adjustment of rates was effected on a different principle. A differential tariff was arrived at, based on distance, under which, taking Chicago

as a fixed point and the rate from that city to New York as the standard, a reduction from it of ten per cent. was allowed in favor of Philadelphia, and one of 12.5 per cent. in favor of Baltimore. The position of Boston was not affected by this arrangement; the old contract being still adhered to, under which, through a rebate in case of export, foreign shipments were made from Boston on the same terms as from New York.

In its practical operation this new system, based as it was on distance in miles to the seaboard, proved highly advantageous to the southern lines. While the difference in their favor was ten and thirteen per cent. from Chicago, from other points it was much larger, until in the case of Cincinnati and Baltimore it became no less than twenty-four per cent. The effect of this soon became apparent in the largely increased receipts of produce at Philadelphia and at Baltimore, indicating an alarming diversion of the export trade from New York; for the difference in rates between the ports was not infrequently almost equal to the entire ocean freight to Europe. When those controlling the New York Central road became fully awake to this fact, and when they also realized the pressure in the way of equal competition under any circumstances which Baltimore and Philadelphia, with all their perfect facilities for handling through business, could now bring to bear upon them, it naturally occurred to them that the time had come for refusing longer to concede a differential rate in favor of those who seemed in no respect less advantageously placed than themselves. In order, however, to assume a consistent position on this subject, it became necessary for the Central road to extend the principle beyond New York, and to claim a uniform rate from the interior to all the seaboard points. This principle it was perfectly obvious that the southern or shorter routes would only concede under a sense of absolute compulsion. A full trial of strength thus became inevitable.

The struggle did not, however, break out in the first place between those who subsequently became the principal parties to it. On the contrary, all through the month of March and the early part of April last, conferences were held and strenuous efforts made to hold the through lines to an understanding among themselves. At the last of these, on the 4th of April, the New York Central represented that it was under the necessity of meeting the competition of the Grand Trunk in New England, and to this those representing the other lines assented upon the understanding that this struggle was to be a local one, and was not to extend to New York, or to divert business from that city. In the course, however, of a few days, it became apparent that the contest could not be thus restricted, and as the result of a final conference on the 18th of April, at which a number of complaints were presented, the New York Central finally gave notice of the complete abandonment of all agreements, and almost immediately a general war of rates began. Between the 3d of May and the 14th of June, the fare between Boston and Chicago over the New York Central fell from $25.85 to $14, and that over the Grand Trunk from $23.85 to 12.50; while as respects freights, the rates between Boston and Chicago on articles of the first class fell from 75 cents per hundred pounds to 20 cents, and those on agricultural products from Chicago to New York fell from 50 cents per hundred to 18 cents. These, also, were the public rates, while innumerable special contracts on terms far more favorable to shippers were made wherever business was competed for. Shippers whose patronage was really worth having were, in fact, in a position to dictate their own terms; and they did it. For six months the spectacle was witnessed of railroads hauling merchandise 1,013 miles east for $3.60 per ton, and the same distance west for $2.80 per ton, —in the one case at the rate of 3.5 mills per ton per mile, and in the other at the rate of 2.8 mills; a result which made sober and reasonable the most extravagant predictions which the advocates of cheap transportation had ever ventured to utter.

No sooner was the struggle fairly developed than the true issue was boldly avowed by the New York Central,—it being to restore the commercial supremacy of New York, imperiled by the rapid development of southern rivals. As a natural result of the mistaken rail-

road policy which has been described, Boston counted for nothing in the struggle,—controlling only locally competing lines, and no single consolidated through line, it was in no position to assert itself, or to defend its own interests. Yet, in fact, the interests of Boston as a commercial point were more deeply involved in the issue of the struggle than those of any other city; for the mileage charge, if persisted in, could only result in transferring the whole business of exporting produce from the northern to the more southern points. Fortunately, on this point, as between New York on the one side and Philadelphia and Baltimore on the other, the interests of New York were identical with those of Boston. The issue was a simple one. It was conceded on all sides that in the case of rival or competing lines between any two given points, as Chicago and New York, the shorter or more direct route had the right, as it was termed, to establish the rate; that is, it fixed a rate, and the longer routes were obliged to meet it, regardless of their own greater mileage, the principle of charging so much per ton per mile being, for obvious reasons, inapplicable. Where, however, lines terminated at different though competing centers, it was maintained that the principle of mileage charge should apply, —that there was no reason, for instance, why Baltimore should not enjoy, as compared with Portland or Boston, the full advantage of its geographical situation. If conceded, this principle could have practically resulted in but one thing: whenever railroads could obtain paying rates, the volume of produce seeking export would have gone irresistibly to the nearer or more southern ports; whenever, on the contrary, rates were very low, the tendency would have been towards the northern ports. This necessarily came from the fact that upon a high or 50 cents per hundred rate, with a mileage difference of ten per cent. in favor of the more southern port, the charge to that port would be as much as 5 cents a hundred less than to the more northern port, which would be sufficient, as experience had shown, to draw the business almost exclusively into that channel. If, on the other hand, rates became very low, falling to 15 cents a hundred, then the difference in favor of the southern points would be but 1.5 cents, and this might very possibly not prove a sufficient inducement to divert the course of trade from its natural outlets through the northern ports. While the managers of the Baltimore & Ohio and the Pennsylvania roads insisted, therefore, on the differential allowance, those of the New York Central met them by fixing rates at so low a point that the differential allowance, when insisted upon, could not amount to enough to influence the course of traffic. It is not yet apparent, so far as the railroads are concerned, what the effect of the process involved in carrying out this method of warfare has been. Before the substitution of steel rails for iron, the roads could not possibly have endured the test. As it was, some idea may be realized of the wonderful economy which has been attained in the movement of merchandise, from the fact that as a regular thing a ton in weight was moved 450 miles from Buffalo to New York for $1.50, whereas in the early part of the century it would have cost $100. Meanwhile, there is some reason to suppose that under certain favorable conditions the transportation of freight even at this rate is not unremunerative to the companies concerned in it. Indeed, judging by their published reports while the recent struggle was going on, it might not unfairly be inferred that throughout it the trunk lines were realizing a quite satisfactory profit on their entire business. Naturally, however, each of the leading competitors then felt obliged to insist that it was suffering least of all, and was, indeed, in a position to continue the struggle indefinitely. Accordingly, they none of them reduced their rates of dividend. In point of fact, however, there can be little doubt that the resources of all, even the strongest of the railroad companies, were heavily strained in carrying on the struggle, while many of the smaller and weaker ones were driven to actual bankruptcy.

Having practically lasted for over eight months, the struggle has been brought to a close while this Report is passing through the press (December 16). Of the arrangement arrived at, and of its bearing on the interests of the several seaboard points, it would, therefore, be somewhat premature to now venture an

opinion. It was based on two distinct principles. A differential rate, computed on mileage distance, was conceded to the southern lines on all shipments from the West to the seaboard for home consumption, while equal rates were to be allowed on all foreign shipments wholly irrespective of the port through which they were made. If, for instance, a car-load of wheat was shipped at Chicago or St. Louis for Baltimore and no further, it was to be carried at a rate of thirteen per cent. in the one case, and fourteen per cent. in the other, less than if it were shipped to New York. If, however, the car-load was originally shipped to Liverpool, it was to be at the same rate for the entire distance, whether it went through New York or through Baltimore; and if, having reached Baltimore or New York at the local rate, it was then shipped to Liverpool, such rebates were to be allowed as would equalize the several ports. As respected local rates, so called, the simple difference between this arrangement and the one which preceded the confliet was, that the excessive difference in favor of Philadelphia and Baltimore on shipments from points south of Chicago was fixed at one given percentage. Under the previous arrangement this had varied from fourteen per cent. reduction from the New York rate in the case of St. Louis shipments to Baltimore to twenty-four per cent. reduction on shipments to the same place from Cincinnati. In place of this, two points and two corresponding differential rates were now fixed,—a concession of ten per cent. from the Chicago-New-York rate was made in favor of Philadelphia, and thirteen per cent. in favor of Baltimore on all Chicago shipments; and another allowance of nine per cent. in favor of Philadelphia, and fourteen per cent. in favor of Baltimore, on all St. Louis, Indianapolis and Cincinnati shipments. The new basis of agreement seems, in fact, designed to secure equality on foreign shipments to all the exporting points, while on local shipments a slight advantage is given to New York in the northern part of the great district in which all the trunk lines compete, and a larger advantage is given to Baltimore in the southern part of that district. Boston, for the reasons already stated, necessarily shares in a fixed proportion in the advantages or disadvantages which ensue under the practical working of the arrangement to New York.

The chief objection to the arrangement is an obvious one. It apparently settles nothing. In operation it cannot but be found too complicated to admit of the parties long abiding by it. The absolute want of faith and of confidence in each other which has hitherto marked the proceedings of those managing the great trunk lines, cannot but find ample field for development in the practical working of a system so intricate. The question of rebates on ocean shipments will admit of infinite wrangling. Apparently, in order to arrive at a decision in each case, it will be necessary to ascertain not only the ocean rate actually paid on that particular shipment, but also what was the rate on the same day to the same destination from the other ports. Practically, such questions, arising the whole time, must prove impossible of decision except by the dictum of some common tribunal, for which no provision is made. This combination, therefore, like the many which have preceded it, lacks, so far as can be judged, the elements essential to its permanence. There is no one either authorized or competent to keep the peace between the high contracting parties. Each reserves the right to construe the agreement to suit itself, and to refuse obedience to the decision of any one else. There is neither a court of common arbitrament, nor, even if there was, do the contracting parties show any disposition to put themselves under sufficient bonds to ensure their acquiescence in its decisions. Without this, all railroad combinations in this country, where a division of territory is impracticable, will prove but temporary. Even were they, under certain conditions, practicable, they are not so now, owing to the fact that the whole complicated system under which through or competitive railroad business is done is curiously vicious and extravagant, and must be radically reformed as a preliminary to any final settlement. It now implies the existence of a vast army of subordinates whose very existence depends on that not being done which those controlling the lines which feed them are continually trying to do. To realize the truth of

this fact, it is but necessary for any person to walk down the leading business streets of any considerable town in the country. He will see that a great number of expensive offices bear the signs of railroad companies and of car and dispatch lines, and at them tickets can be purchased and rates of freight made which are binding on all the companies included. The rents, salaries and perquisites of this army of retainers all come out of the railroad corporations, and the interests of the retainers and the corporations are exactly antagonistic, —the first are always working to bring about railroad wars, in which business with them is brisk, while the last are always striving to effect combinations.

As long as this state of affairs continues, periodic railroad wars will continue. The hopes of stockholders and the fears of the business public in regard to their ceasing will be equally disappointed. A conference of those controlling the trunk lines which began its labors by clearing away the whole complicated machinery through which competitive business is fought over and secured, and then completed them by establishing a common board of arbitrament over points of dispute, clothed with a real executory power,—such a conference might result in something For this, however, no one seems as yet to be ready, and the trials of strength must, therefore, continue. Meanwhile, each year the results of the attempts at combination become weaker and verge more nearly on the ludicrous, while the wars become longer and sharper and the resulting rates permanently lower. It is not probable, however, that the recent conflict will be immediately renewed. The severe losses and bitter experience of the last few months will not be forgotten at once, and for a time matters of dispute will remain unsettled, and breaches of compact will pass unnoticed. Meanwhile, so far as the railroad system of the whole country is concerned, it is necessary to bear in mind that these continually renewed struggles between the great continental and competing trunk lines are but incidents in a phase of the process of development. The railroad interest of the country is consolidating, and it is doing it through the survivorship of the strongest. Each new war of rates is made more severe than that which preceded it, and the weaker corporations find themselves continually nearer the end of their resources, and less able to sustain the pressure. As they succumb under it, they are absorbed through bankruptcy by the yet solvent lines, as fast as these see their advantage in absorbing them. Competition is, therefore, as rapidly as possible resulting in consolidation, and this process seems likely to continue indefinitely through the immediate future. What shape this consolidation will ultimately assume, in another and later phase of development, it is futile now to consider.

Returning, therefore, to the combination of December 16, and its probable effect on the interests of Massachusetts and of Boston, the members of this Board see no reason to modify the conclusions they have heretofore expressed. There seems to be no real ground for apprehending local disadvantage from any railroad combination which has been or is likely to be effected. On this subject they, a year ago, expressed themselves as follows:—"However it may be under exceptional circumstances and for brief periods, in the long run active competition between the through routes, cannot but be prejudicial to Massachusetts' interests. It leads directly to discrimination in favor of rival communities. It does so for the obvious reason that, as a rule, railroad competition is and must continue to be stronger to New York and other seaboard points than to Boston. . . . . .

"In the struggle of competition, therefore, Boston stands in a poorer position to protect itself than any other seaboard city. In the long run, the discrimination will surely be against it, in the future as in the past.

"It would seem, therefore, to be the true policy of this section to encourage, rather than to discourage, a general public combination of the through railroad routes, based on principles of equality and stability. The law of the strongest does not work in our favor, and we cannot permanently steal business. Before a permanent combination is arrived at, however, there are certain principles the concession of which, as a part of the accepted policy of any general railroad system, is essential. Foremost among

these is the absolute equality of the Atlantic seaboard centers as respects the movement of merchandise to and from certain of the great distributing points of the West."

It has been seen, that in this last essential respect, the result arrived at on December 16 is in the nature of a compromise—the principle of equal rates on produce for export being acceded to on the one hand, while for local business a differential rate is conceded on the other. The final solution of the controversies between the great through lines is, however, yet to be arrived at. This compromise, however, is most unlikely to prove a final solution of the controversies between the great through lines, for the simple reason that it must, apparently in practice, work adversely to some of them. New York is not yet ready to see its commercial supremacy pass away from it; nor will Philadelphia and Baltimore quietly surrender the commercial advantages they have so hardly won. But the experience of the last few years would seem to warrant a conclusion that New York, in this matter, can no longer afford to concede anything,—that those having charge of the interests of that city must insist on absolute seaboard equality. In that case, the present truce is simply to permit the trial of an experiment, the result of which will be a renewal of hostilities over the old issue. Did Massachusetts and Boston possess an independent line, which could be wielded exclusively in their interests, they would unquestionably be strongly disposed to insist on the full enjoyment of all the great natural and geographical advantages of their city, as a recognized part of any permanent settlement. Under the circumstances, however, they are wholly dependent on such chance and suicidal competition as the Grand Trunk Railway may be disposed to wage with its more powerful and direct rivals. This in the future is unlikely to prove so active an element in the problem as in the past. The Grand Trunk, being financially the weakest of the through lines, has apparently suffered in the recent conflict much the most severely of them all, and a continuance of it would not improbably result in plunging that company more deeply than at present into bankruptcy. Its connections, also, in New England have felt the strain very severely, and have neither the means nor the disposition to endure it longer.

Finally, the present situation, and the conclusions which may apparently with some degree of confidence be deduced from it, can be briefly summed up as follows: As respects what is known as through business, the period of active railroad competition has of late entered on a new phase,—that of competition between rival termini instead of between rival lines to the same terminus. In the course of this struggle, so far as Massachusetts and Boston are concerned, an equality of advantages with other localities seems to be secured through a community of their interests with those of New York, and it must depend upon our local railroads, and especially the business community, to afford such facilities of handling and of transit as will constitute an inducement to traffic to seek this channel. The vital advantage of an independent thoroughfare, on which the whole future of Philadelphia and Baltimore seems to so great a degree to depend, is lost to Boston, and under present circumstances it is very difficult to see how it can be recovered. Indeed, the true policy for this community to adopt would not seem to dictate even an endeavor to recover it, for it is as useless to attempt at certain stages of development to abandon a long-established plan and to adopt a new one, as it is to stand still and repine over lost opportunities. Good or bad, the policy of Massachusetts as respects its through railroad connections has been adopted, and more than twenty years of time and twenty millions of the public money have been expended in the attempt to carry it out. It only remains to do so logically, with as much foresight and at as little outlay as possible. To bring anything to pass, however, a definite plan is essential; not only must there be a distinct end in view, but all efforts must be concentrated on the attainment of that end. Now, the fundamental idea of the recent Massachusetts' policy has been local railroad competition,—the same local competition, whether to the Hoosac Tunnel "toll-gate," or to Albany or to Ogdensburg. The horizon has always been a limited one, proper to the conditions

which existed a quarter of a century ago. To bring anything about, it must be extended. The results accomplished in their own interests by other communities should, as far as possible, be appropriated for New England. A local competition must give place to a continental competition,—the bounds must be transferred from the Hudson to the Mississippi. This can now only be done by drawing the great thoroughfares to the other seaboard cities into New England. The Hoosac Tunnel, instead of being a "toll-gate," must become part of a trunk line, and that trunk line, instead of competing at Albany with the Boston & Albany for the traffic which flows over the New York Central, must be extended to the interior, bringing to us the whole competing force of the Erie and the Pennsylvania, and even the Baltimore and Ohio. This subject, however, has already been discussed in an official report in connection with the tunnel line made to a previous Legislature, and, as it more especially relates to the best way in which that line can be utilized, its further discussion at this time would seem to belong rather to those to whom the management of the tunnel "toll-gate" is at present intrusted, than to the members of this Board.

CHAS. F. ADAMS, JR.,
A. D. BRIGGS,
FRANCIS M JOHNSON,
Railroad Commissioners.

VENTILATION OF THE HOOSAC TUNNEL.*

BY THOMAS DOANE.

THERE is very little published information concerning tunnels.

Mr. Charles S. Storrow says, (Senate Document, No. 93, year 1863, page 21 of appendix), very little has been published in England upon the subject. Mr. Forrest, the Secretary of the Institution of Civil Engineers in London, who kindly assisted me in my enquiries, ransacked the Library of the Institution for me, and took down everything he thought would prove interesting, but the list was lamentably small, and the accounts meager. The most complete English work was entitled "Practical Tunnelling" by F. W. Simms, which gives full description of two important works.

Mr. Storrow also says, page 59 of said appendix: The *only* French Tunnel of which I could find any published description, was the Tunnel of St. Martin D'estraux.

It is probable that the report of the Commissioners upon The Troy and Greenfield Railroad, & Hoosac Tunnel, above referred to as Senate Doc. 93, year 1863, with the appendix, etc., is the best work extant upon Tunnels. To this, therefore, we are mainly confined, as to the ventilation of tunnels, while the ventilation of Mines may aid somewhat in coming to conclusions.

It may be taken as a *fact*, that the Hoosac Tunnel will require ventilation. It is probable that locomotives of the present mode of construction will be used in the tunnel more or less at least. The grades rise from both ends to the center of the tunnel, at the rate of about 26⅓ to the mile. An engine passing *up* these grades must discharge large quantities of smoke, but very little while passing down. If there are many trains, and many must be expected, or the tunnel has been built in vain, they are passing in *both* directions and must discharge smoke through all the portions of the tunnel, about equally.

If smoke-consuming engines are used, like the Weston or others which may be invented, there are invisible resultants of combustion, like carbonic acid, and sulphurous gases, which must be removed from the tunnel. This is necessary in order to procure a *pleasant* as well as a *wholesome* atmosphere for those who have the care of the tunnel, and for those who shall pass through either upon freight or passenger trains.

Still another reason for ventilation is that the dust and dirt resulting from

* A paper read before the Boston Society of Civil Engineers.

smoke and from the traffic through the tunnel, so far as it may for a time float in the air, may be carried out of the tunnel. The tunnel has been open for business, and that of small amount, for about one year, and already the sides and floor have a thick covering of black dust and soot. Some of this and perhaps the most of the fine black portion comes from the lamp of the workmen engaged in lining the tunnel, in which kerosene oil is burnt, and which will not perhaps be used after the tunnel is finished.

There will, however, be large deposits of solids from smoke, unless smoke consuming engines are used, and a large portion of these can be got rid of by rapid ventilation. Unless this is done, there will be large accumulations of filth in so long a tunnel, where fires cannot, before entering the tunnel be prepared for a shoot through, as in short ones.

There is still another reason why the *maximum* of ventilation should be provided for and maintained.

The masonry with which the tunnel is lined, is not entirely, nor is it largely, made up of hard brick. The bricks used absorb of water from fifteen to thirty-two per cent. of their cubic capacity. Many portions of the rock tunnel are very wet, all of it more or less so, and therefore, if the masonry on the unprotected rock is exposed in this saturated condition to freezing, the masonry will scale, and the rock will be thrown down. Even, if through the protection of doors, *frost* does not reach inside the tunnel, the continual presence of water will carry on gradual decomposition, already begun. The air of the tunnel in a *quiet state* is full of moisture up to the dew-point, but, if ventilation as rapid as possible be kept up, the masonry and the rock will, upon their surfaces, and to some extent within, be brought into their dryest possible condition, and, therefore, into their safest and most durable state.

Again, it has been proved by experience, that a train can be hauled up a grade outside of the tunnel of forty-two feet to the mile, easier than upon the grades of 26$\frac{4}{5}$ feet per mile within the tunnel. This may be accounted for in many ways, among which are the following: While workmen are engaged in lining the tunnel, oil and tallow are spilled upon the rails, preventing the driving wheels from getting hold of the rails. Again, while passing under the arches in process of construction, and among the men, the speed of trains has to be reduced, and sometimes they are brought almost or quite to a stand, thus involving irregularity of speed, and greater strain upon the engine, in passing hard spots. But there can be no question, that the large amount of moisture upon the rails preventing adhesion, and the closer condition of the air, preventing combustion, have much to do in making the easier grades *within* harder to surmount than much heavier grades *without* the tunnel.

Once more, the tunnel is sufficiently *dark*, under the best of circumstances. When filled with smoke, or with vapor, it sometimes becomes impossible for workmen to keep about their business, and the head light of a locomotive will not show the track ten feet in advance of the pilot. Workmen should be able to see the light of any approaching train some distance away, and it is of the utmost importance that the track should be visible to the engine man many hundred feet ahead of his train. The great danger to trains in the tunnel will not be from rocks falling upon the train, but from running the trains upon rock previously fallen upon the track. It is to be presumed that few rocks falling from the roof will be sufficiently large to derail the train, while a small piece lying upon or near the rails, would throw off, and wreck a train; and, besides, any given train passing the tunnel once a day, as in any given spot under a weak roof but a moment of time, while gravity is at work upon this weak roof all of the twenty-four hours, and may much more probably result in a fall and a wreck, because of the great difference of time in which the exposure is of one *part* or the other. It is, therefore, important in *this view* to maintain the largest amount of ventilation possible.

The reasons for abundant ventilation then, may be recapitulated as follows:

1st. To remove *smoke* and *gases* from common engines.
2d. To remove gases from smoke consuming engines.
3d. To carry out of the tunnel before deposits the *solids* of smoke.

4th. To preserve the atmosphere in a *pure* condition, so that it shall be *pleasant* and *wholesome* for employés and passengers.

5th. To preserve the tunnel in as *cleanly* a condition as possible.

6th. To preserve the brick masonry, of which there will be about two miles in the tunnel, in as *safe* and *dry* a condition as possible, and to prevent scaling by frost.

7th. To keep the exposed rock of the other three miles in a *dry* state, and thus save it from falling or exposure to frost.

8th. To provide and maintain a rail of greatest possible *adhesion*.

9th. To maintain a *transparent* air, for the workmen in care of the tunnel and track.

10th. To maintain a transparent air for the train men, so that accidents to trains be reduced to the minimum.

TESTIMONY OF EXPERTS AS TO THE NECESSITY OF CENTRAL SHAFT FOR VENTILATION.

From Senate Doc. 93, year 1863, page 51, we learn the opinion of the Commission, John W. Brooks, S. M. Felton and Alexander Holmes, the first two of whom are eminent as Civil Engineers, when they say they regard "the (central) shaft as likely to be the main dependence for ventilation."

They again say, upon page 52, "as the (central) shaft will most likely become the main reliance for ventilation, it should be of liberal dimensions and we would recommend it to be of an area equal to a circle of twenty feet diameter."

At page 35 they say, "the character (of the Hoosac Tunnel) is so unusual, that they have not felt at liberty to neglect any means of inquiry which promised to give them light in regard to it;" and also at same page, "the railroad tunnels in this country, which from the nature of the ground cannot be provided with ventilating shafts when necessary, at moderate expense, are so short, that the question of ventilation has not necessarily been of much influence in determining their character. In the Hoosac, this is one of the great questions for consideration." At page 47, they say, "It is not unusual for tunnels in Europe to have a summit within them, where the track is a few feet lower than the roof at the highest end, but Mr. Storrow heard of no case like the Hoosac."

CENTRAL SHAFT INDISPENSABLE FOR VENTILATION.

From Commissioners report, Senate Doc. 93, year 1863, page 49, we find that great care has been taken to obtain the opinions of the best authorities in England and on the Continent among those of the most enlarged experience, both as to the construction and use of long tunnels. . . . "These authorities are all perfectly agreed that a shaft in the center is indispensable."

Mr. Storrow obtained the views of many more persons qualified to judge, than he has named in his report.

At page 34, of appendix, to said Doc. 93, we find Mr. Brotherhead, who is not only a contractor of high character and standing, but also the owner of a very extensive machine shop and forge works, saying, "unhesitatingly that there *should be a shaft* in the center at the summit," and on page 36, that it should be for ventilation.

On page 42 of appendix, Mr. Michael Lane, Engineer, a gentleman of very great practical experience, and long association with Mr. Brunel, and, with important works upon the Tunnel under the Thames at London, says: "The central shaft would certainly be required, both to save time and furnish ventilation, for with a summit within the tunnel it was *indispensable* for the latter purpose if necessary for the other."

At page 42 of appendix again, we find that Mr. Sacré, Engineer of the Woodhead Tunnel, nearly as long as the Hoosac, Mr. Richardson, Engineer of the Bristol and South Wales Junction Railway, Mr. Mc Clean, and his partner Mr. Stileman, who enlarged the Lindal Tunnel, both distinguished engineers, Mr. Watson, a gentleman connected with Sir Morton Peto and Mr. Betts, and Mr. Thomas Brassy, "all concur in the opinion that . . . the central shaft with such a line as ours is indispensable."

At page 74 of appendix, Mr. Storrow says: "In conversing with French engineers in relation to the Hoosac tunnel, I

found a most decided opinion that the central shaft would be indispensable."

At page 113 of appendix, Mr. Sommeiller, one of the engineers of the Mont Cenis Tunnel, says: "A central shaft for our line, he thought, indispensable, and . . . felt that in the possibility of such a shaft, we had an infinite advantage over their own work, where such an aid either for construction or ventilation, is altogether beyond their reach."

At page 118 of appendix, Mr. Storrow, in summing up the conclusions to which he has arrived, says: "Believing as I do that a central shaft is *indispensable*, I assume, of course, that it will be constructed."

On page 131 of said appendix, Mr. Benj. W. Latrobe, of Baltimore, who has had great experience in tunneling, says: "These shafts (for on same page he proposes two) I consider *essential* for the three-fold purposes of expediting the work, insuring its correct alignment, and for better ventilation;" and at page 137 he estimates for two (2) circular shafts of twenty feet diameter each.

On page 187 of appendix, Mr. James Laurie, of Hartford, an engineer of large experience, says: "Still, in a tunnel of the great length of the Hoosac, with a summit in the middle and the entrance nearly at the same level, there may be doubts whether, with numerous trains passing through it, a speedy and perfect ventilation would be established. Any fears on this point could be overcome by sinking a shaft near the center. The ventilation could then be regulated if necessary, by the common expedient in mining regions of keeping a fire burning near the top or bottom of the shaft."

The testimony of the above-named distinguished engineers and contractors, and the universal opinion among all consulted, pointing to the *absolute necessity* of a central shaft for *ventilation*, is dwelt upon at this length, because there are some styling themselves *practical* men, who deny the necessity of the shaft for *this* purpose, and claim that it should be *closed up*.

The testimony, unless it can be refuted by actual experience in the Hoosac Tunnel now open through, should be conclusive in determining whether to arrange the central shaft for ventilation or to close it up permanently.

It cannot be thus refuted. During the past winter, and in cold weather, an attempt has been made in a small way to test this question. The central shaft has remained closed near both its top and bottom by timber floors imperfectly laid, and leaking about their edges and through the cracks in the boards. In these floors are traps about four feet by six feet, equal to twenty-four square feet. It was proposed to close these trap doors and let them remain until it came to the knowledge of the many workmen in the tunnel, by reason of the accumulation of bad air, and then to open them and the whole shaft fully. It was further proposed to make observations as to temperature, direction of current of air, smokiness or clearness, passage of trains, direction of trains, direction of outside winds, &c., &c., at each end, at central shaft, and at two points each half way between the ends and central shaft during the time of closed shaft and open shaft. These latter data, though with only the trap door open and not the whole shaft open, and taken by observers walking through instead of by separate observers simultaneously, have been furnished me. But the shaft did not remain closed long before there was a demand on the part of the workmen to have it opened, when before they had deemed it unessential and had even claimed, contrary to the fact, that it had been closed all winter. It should be remembered that during the progress of the work, too, of enlarging and lining the tunnel, many men have been so overcome by the impure air, as to become helpless, when they were taken out of the tunnel upon cars. The question may be raised, why did they not secure better ventilation if it could be had, and in reply it may be said that the strong drafts thus raised, were uncomfortable to some of the men, inconvenient in causing lights to flare, besides making the tunnel so cold as to interfere in the making and use of mortar.

Some of the natural circumstances by which the Hoosac Tunnel is surrounded are unfavorable to natural ventilation without a shaft. The magnetic bearing of the line of the tunnel is N. 80° 25′ N, and its true bearing, when corrected for

variation of the needle, almost exactly East and West.

The elevations of the two ends of the tunnel are very nearly alike, and both ends are about equally exposed to sunlight and heat, the one in the morning, the other in the evening.

In *most* European tunnels, the grades are rising in one direction entirely through them, and sometimes at a rapid rate. They thus become an inclined chimney, and being short, manage to ventilate after a fashion.

In the Mont Cenis Tunnel, which more nearly corresponds in magnitude to the Hoosac, than any other completed, natural circumstances, in combination with the grades, led its projectors to expect natural ventilation without a shaft, which in their case was impossible, and so far as known, they are very little disappointed in their expectations.

At page 110 of said appendix, they say, as regards ventilation, "after the tunnel shall have been completed, they have no fears, and do not expect to require the aid of artificial means.

"The completed railways on both sides of the tunnel, and with which it is to be connected are single track railways. Yet they have insisted upon a tunnel large enough for a double track. Whether one or two tracks shall eventually be laid, they consider this section as the smallest that would answer for purposes of ventilation. They mainly depend upon a natural current of air, which they expect through it. Modane is in a cold valley on the *North* side of the mountain. Bardonneche is a much warmer valley on the *South* side."

Mr. Grattoni said, "that observations which have been continued for three years, show that there is almost universally a great difference in the temperatures between the two ends, and also in barometric heights."

Taking also into consideration the prevailing directions of the winds, and the fact that the Bardonneche end, "besides being the warmer end," is 435 ft. higher than the other, they see reason to expect an almost constant current from north to south through the tunnel.

In the *Engineer*, of January 21st, 1876, Vol. 41 page 40, Mr. G. J. Marrison, in a paper read at London, says that: "at the Mt. Cenis Tunnel, there are now sixteen trains per day, and there is not much urgency for better ventilation, but that if there were twenty trains per day, mechanical ventilation would probably be an absolute necessity."

The area of the section of the Mont Cenis Tunnel above top of ties, and consequently area of ventilation as nearly as can be estimated is $457\frac{83}{100}$ square feet. In form, its curves of shortest radius is at the crown, having in this respect the advantage in ventilation over the Hoosac, where the curve of *longest* radius is at the crown. Its center height is twenty-one feet above ties, while the Hoosac is but $18\frac{7}{10}$ feet.

That there will be smoke or deleterious gases is in the tunnel, which should be taken away, there can be but little doubt.

Mr. Storrow, at page 15 of appendix, says of the Sopperton Tunnel, which is one mile long with a grade all the way through in one direction of $75\frac{13}{100}$ ft. per mile: "As the tunnel now is, I was told that the men work in it all day whenever necessary, although they do not like to, but that if two (2) freight trains should follow each other up without the smoke being cleared away, it would be very difficult for them to work, and if four (4) trains followed each other, it would be impossible." And again page 16: "I stood a few yards outside of the tunnel when a heavy coal train passed up and came out of it with an additional engine pushing in the rear; the smoke and vapor were extremely thick, and as seen from the outside the whole entrance of the tunnel was a black mass." This tunnel is twenty-eight feet wide and twenty feet high.

Mr. Latrobe says, page 127 appendix, that in the longer tunnels such as the Kingwood on the Baltimore and Ohio R. R., (4100 ft. long), "inconvenience is at times felt at the smoke of trains;" and again, "the smoke and gas affected, at times, the workmen engaged in arching the Kingwood and Broadten tunnels on the Baltimore and Ohio R. R. so as to make it expedient, in connection with other interruptions to the work, to resume the use of the temporary tracks over the tops of the ridges."

Ventilation of some sort and to some extent being requisite, it will be best to be rid of the smoke as *early* as possible.

Without a central shaft, the entire cubical contents of the tunnel must pass one way or the other, entirely through the tunnel. If there were a central shaft *operative*, the tunnel would be ventilated in one half the time, or rather, there would be but *one half* the amount of bad air to pass through either section of the tunnel.

Judging by the limited experience already had in the Hoosac Tunnel, we conclude the weight of evidence to be on the side of the necessity of maintaining ventilation through the central shaft. Though there are about 400 men with half as many lights, at work vitiating the air, there are but few trains through the tunnel. These are confined to the use of wood in the engines, and even with every precaution and twenty-four square feet open in the shaft, the men are often compelled to descend from their work in the roof. There are also very many occasions when, with the best of lights, sight will not reach beyond a few feet.

It is, therefore, taken to be proved, both by testimony, experience and common sense, that the *central shaft* is *indispensable to ventilation.*

The next question is *how to make it most useful?*

The data given are the following:

The size prepared for the central shaft (Senate Doc. 93, year 1863, page 52 of report and page 137 of appendix) was equal to a circle of 20 feet diameter, which is $314\frac{188}{1000}$ square feet. The shape and size decided upon was an ellipse, 27 feet by 15 feet whose area is $318\frac{088}{1000}$ square feet.

Its location is *over* the tunnel, its major axis lying parallel to and over foot north of the center line of the tunnel. The section of the tunnel above top of ties is as follows: in solid rock, 24 feet wide and 18.71 high, containing $409\frac{18}{100}$ square feet. In rock where lined with masonry with vertical sides, 26 feet wide and 21.21 high, containg $478\frac{88}{100}$ square feet. In soft rock where lined with masonry and with invert, 26 feet wide, 22.71 feet high, containing $505\frac{338}{1000}$ square feet.

The grades are as follows: beginning at the east end, there are 2118 feet of grade rising at the rate of 18.48 feet per mile, then 10500 feet rising at the rate of 26.40 per mile, then 219 feet of level to the central shaft, then 115 feet more of level, then 12129 feet, falling at the rate of 26.40 feet per mile to the west end. The elevation of tie-grade at east end is 768.87 feet above tide, of tie-grade at central shaft 828.81 feet, and of west end 768.41 feet. The height of east portal roof above tie grade, as before stated, 18.71 feet, and of west portal roof is 22.71 feet.

The difference of level between tie grade at east end and central shaft is $59\frac{94}{100}$, and between west end and central shaft is $60\frac{40}{100}$ feet. A level line drawn from the roof of the east portal, will strike grade at a distance of 4376 feet within the east portal; and a similar level line from the roof of west portal, will strike grade at a distance of 4542 feet within.

The following diagram will illustrate this:

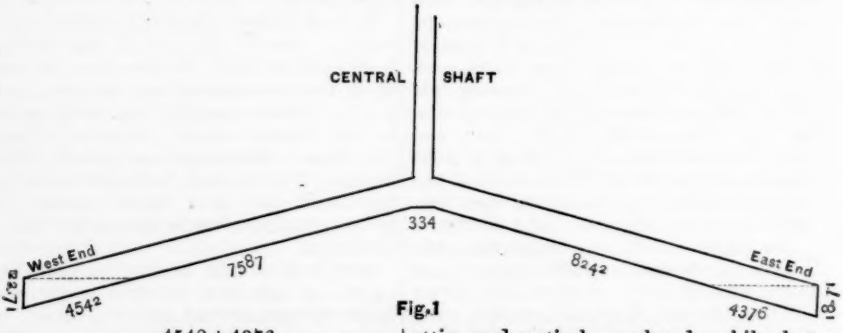

Fig. 1

Here we have $\frac{4542+4376}{2}+7587+334+8242=20622$ feet of the length of the tunnel lying in what may be called an attic, and entirely enclosed, while but $\frac{4542+4376}{2}=4459$ feet are exposed to ventilation through the portals. There

is then only about $\frac{1}{6}\left(\text{exactly } \frac{4459}{25081}\right)$ of the length of the tunnel, and consequently but one-sixth of the volume of the tunnel, calling the section a parallelogram as much of it is very nearly, that can be ventilated through the portals without the aid of the central shaft, or of an exterior wind flowing either eastwardly or westwardly, through the tunnel. It is plain enough that the direction and force of the winds cannot be relied upon, and therefore that some other means must be used.

Will the forcing in of compressed air, by means of the water power and compressor at the east end, answer the purpose? It has been suggested that *this* will be necessary. A little calculation will show what can be done in this way. The compressor, No. 1, which was used for seven years at the east end, has four cylinders, 13 inches diameter, and 20 inches stroke each, and make sixty revolutions per minute. It will furnish 363 cubic feet of air per minute. The average area of the tunnel may be taken at 464 square feet, so that by this process $\frac{363}{464}$ feet$=\frac{78}{100}$ foot per minute will be ventilated, and this represents the velocity of the air current. As the tunnel is 25,081 feet long, it will take 32,155 minutes = 22 days and 4 hours of 24 hours each to ventilate it *once* through. The tunnel then can be ventilated in this way about once a month provided all trains be stopped during that time.

Should the atmosphere, however, prove so bad, under a heavy traffic, and with the best of ventilation possible, as to be unendurable to the workmen, a pipe of compressed air discharging where work is going on, may become necessary. It would not be wiser to attempt ventilation by fans *alone* at the *ends*, and therefore the only resort is to a shaft opening at the center into the top of the attic, and this, fortunately has been provided by those who have gone before.

At page 34 of said appendix, Mr. Brotherhead says, unhesitatingly, "that there should be a shaft in the center of the Hoosac Tunnel," at the summit, and from its position and height he anticipates a great upward draft in it, *independently* of *artificial means.*

At page 45 of appendix, Mr. Storrow, in summing up his observations on tunnels in England, says: "in no one have *artificial means* of ventilation been required after their construction."

Since Mr. Storrow's visit, which was in 1862, a few tunnels in England have their ventilation *artificially* aided.

What is the testimony concerning the size necessary for a shaft? At page 23 of said appendix, we find that at the Kilsby Tunnel in England, "In May, 1836, one of the large ventilating shafts was commenced and completed in about twelve months. This shaft is sixty feet in diameter and 132 feet deep. The second ventilating shaft is not so deep by thirty feet. The great ventilating shafts, . . . are found fully to answer the purpose for which they were intended, leaving the tunnel entirely free from any offensive vapor immediately after the transit of each train." This tunnel is 7269 feet, or $1\frac{38}{100}$ miles long. The area of *each* shaft is 2827 square feet, or about nine times as great as that of the central shaft of Hoosac Tunnel.

In view of the fact that an ample operative central shaft would in itself reduce the smokiness, either as to density or duration, of the tunnel, as before stated, one-half, because the smoke from either end passes up the shaft instead of going through, or the fresh air coming down flows both ways instead of one, it becomes important to have it large. We cannot have it larger than it now is, but shall even this be reduced is the question.

It would seem that a shaft having not less than double the area of the tunnel would be the best, for then the current of air from the tunnel both ways *up* the shaft, or down the shaft and both ways in the tunnel would maintain equal velocities in both shaft and tunnel. But instead of 928 square feet as the sum of both east and west tunnel areas, we have 318 square feet in the shaft=about one-third.

Any reduction of area of the shaft involves an increased velocity to carry a given amount of air, or a reduction of the volume if the velocity is not increased. The latter will doubtless prove the truth.

It has been suggested that a contraction of the throat of the shaft would aid ventilation, but aside from the above objection it must be remembered that during the winter the shaft will be an upcast and during the summer a downcast, and therefore that both ends of the shaft must be reduced to form throats for the opposite currents. It has never been proved that a reduction for a throat has any value, except to pass the air through fires for the purposes of blowing them or of preventing the escape of smoke from house chimneys into their rooms. If the shaft be reduced at both its ends, it will be a practical reduction of its area to that of its smallest section and the shaft to that extent will have been built in vain. In proof of this is the statement in "Rankine's Steam Engineering," page 287: "It appears that in using this formula, a conical or pyramidal chimney may, without sensible error, be trusted as if it were cylindrical, or prismatic, with an uniform sectional area equal to that of the opening at the top," which is no doubt its smallest area.

Again, D'Aubuissoin says, at page 220, article 203: "Notwithstanding the great irregularities presented by these experiments, they are very remarkable, principally because they exhibit, in a very prominent manner the effect of *enlargement* existing in a conduit, an effect quite as prejudicial as that of *contraction* taken above a certain limit."

Again, "Rankine's Steam Engineering," page 572, the area of the narrowest part of the contracted vein is in every case to be considered as the vertical or effective outlet.

This evidence leads to the belief that in a flue especially when intended to be effective in opposite directions, there should be uniformity and smoothness of bore throughout, and that if this is not attained *the effective* area or dimensions of the shaft is its *smallest* area.

This shows also the importance of not only securing a clear area of flue equal to the theoretical, but that as few excavations as possible be made beyond the theoretical or construction lines, as such enlargements are prejudicial to the flow.

It is a law of flowage, that the resistance from friction *increases* as the square of the velocity of the current. With the *present* ratio between the area of shaft and tunnel, which is about as 1 to 3, the resistance in the shaft will be nine times as much as in the tunnel, and it is certainly unwise to increase the friction, and retard the flow by a further reduction of the area of the shaft.

D'Aubuissoin, at page 217, says: "To give an exact idea of the resistance which contractions of the section of a conduit for a very short extent oppose to motion, suppose that in a pipe we place perpendicularly to its axis a diaphragm, or thin plate, pierced with an orifice.

"When the fluid (or gas) in motion arrives at this, the vein will be contracted, and will also be reduced to the size of the opening. It is through such an opening, thus reduced that it is necessary to force its passage, by taking a velocity as much greater as the opening is smaller, and this velocity will always be superior to that which would take place in this part of the pipe, without the diaphragm.

The excess of force necessary to produce the excess of velocity, the direction of the motion remaining the same, will evidently be the effect of the sudden contraction; it will be the resistance opposed.

It is generally conceded that the central shaft, in order to secure the safety of trains and persons passing beneath it, must be closed with masonry and covered with some material which shall consume the force of stones falling into it. In what manner, then, shall the shaft be connected with the tunnel for the purpose of ventilation?

An opening or openings in the *center of the roof* connecting as directly as possible with the shaft would seem to be the best way. But this method is open in a slight degree to the same objection which lies against an open central shaft, that stones may fall from it.

It may, therefore, be best to place these openings at one side of the tunnel and excavate the flues down to the floor of the tunnel.

It also seems plain that two (2) connecting flues should be made instead of one, for the single flue must incline from the shaft either towards the one end of the tunnel or the other, in order to pass the arching and give an advantage in

ventilation to one or the other, when they should be treated alike.

If there are two flues, the area of each should be not less than one-half that of the shaft, which one-half is equal to $159\frac{04}{100}$ square feet.

On page 204 of D'Aubuissoin, we find that the resistance to flow, or friction, is in the *inverse* ratio of *diameters*. For this reason, and because the connecting flues must be indirect, some increase of area must be made to get the benefit of the full section of the shaft. It is advised to give each flue an area of 180 square feet.

The next question arising is, what shall be the linear direction or alignment of the flues? It has been proposed to begin them at equal elevations, say fifty feet above the roof of the tunnel, upon the opposite sides of the shaft, drive them horizontally east and west about thirty feet each, and then sink them vertically to the tunnel thus:

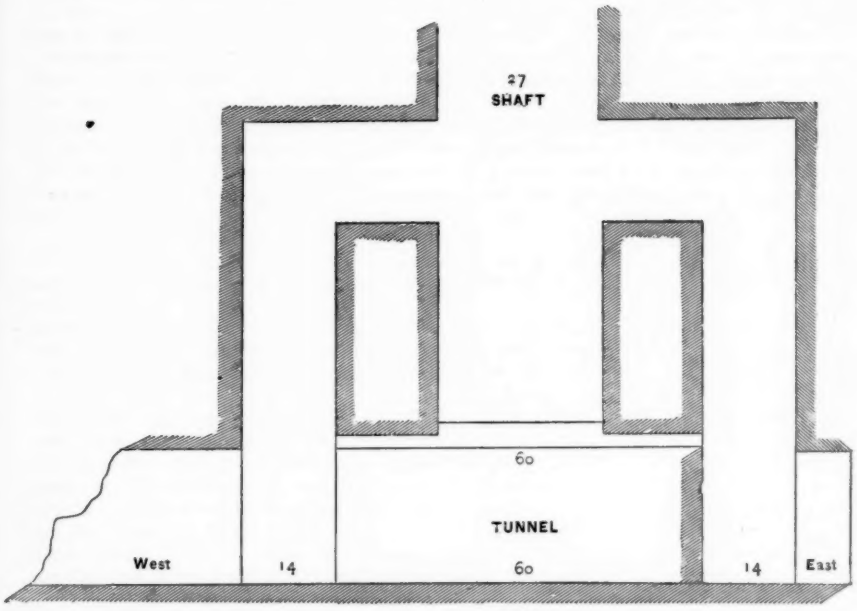

Fig. 2

Scale, $\frac{1}{200}$.

In a practical treatise on ventilation by Morrill Wyman, he states at page 122, that "in all cases of flues entering a chimney, they should be so arranged that the smoke shall assume a direction approaching that of the axis of the chimney into which they enter;" and at page 149, that "two current sentering at right angles of equal velocity destroy each other . . . this should be prevented by giving all flues a *vertical* direction as they enter the chimney."

Again D'Aubuissoin, at page 216, says: "If the effect of well-rounded curves is insensible, it would not be so with angles properly so-called. An experiment of Venoturi shows their influences." And again: "The bad effect of angles is still more manifest in the experiments of Rennie (196); with his pipe fifteen feet long and one-half inch diameter, under a head of four feet, he had per minute a discharge from the straight pipe, 0.4196 cubic feet.

From the pipe with 15 semi-circular bends (=7½ circles) 0.3694 cub. ft.

From the pipe with 1 right angle (=¼ circle) 0.3334 cub. ft.
From the pipe with 24 right angles (=6 circles) 0.1519 cub. ft.

So that a single angle of 90° reduced the discharge more than fifteen curves (one-half circle). This single fact shows with what care all angles should be avoided in the establishment of conduits."

The plan proposed, as will be seen by reference to the foregoing diagram, involves three vertical right angles, besides two horizontal ones, and the discharge of the two opposite flowing currents of presumably equal velocities, directly in the face of each other, and therefore for either reason, and most certainly for both, must be abandoned. The two horizontal right angles, and the lower vertical right angle, will necessarily be retained in the plan to be proposed, but the legs of the horizontal one are so short that the necessary rounding of the rock in excavating will destroy much of their harmful influence.

In order then to preserve in the tunnel the individuality of the currents of the two flues, their openings should be separated from each other by quite a space: in order to avoid the turning of more angles than absolutely necessary, there should be the introduction of a curved alignment, and in order to prevent the collision or fighting in the shaft of the two currents, a partition or brattice should be built up so far that the currents will at its top have attained to a direction in line with the axis of the shaft, and that this direction may be more readily attained, the flues should enter the shaft at as small an angle as possible.

The method proposed in the following diagram, seems more nearly than any other, to meet all their requirements:

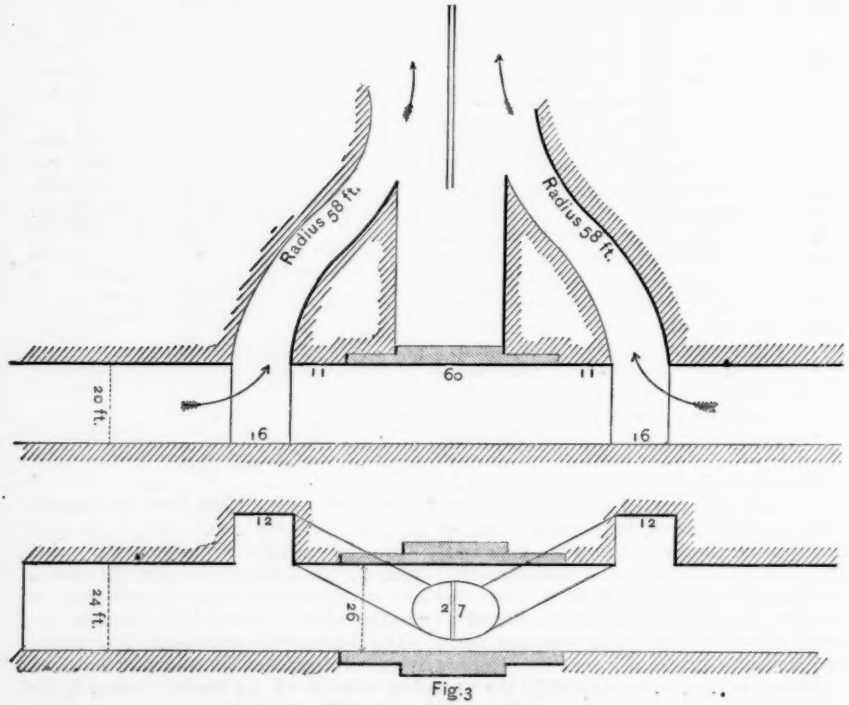

Fig. 3

PLAN AND ELEVATION OF TUNNEL SHAFT.

Scale, $\frac{1}{300}$.

An attempt has been made to get some data in the tunnel, which might help in solving the problem. The failure to make the observations simultaneous at the several stations has destroyed their value in some measure, for while a single observer walked through the tunnel ten times, in an average time of two hours and twenty minutes, there was opportunity for the smoke to follow or precede him, and for marked changes to occur, which could only be properly noted by a stationary observer.

However, some things were found which may be interesting in this connection. The observations were made between February 26th, and March 29th 1876, it *being Winter*.

It is found, with perhaps two slight exceptions which may have been errors, that there was a very uniform increase in temperature from each end towards, and to the central shaft. This was true both while the shaft was closed entirely, and when the small trap door was open, and whichever way the draft was. This indicates the truth of the position taken on page 364, that the grades descending both ways from an attic in the tunnel which will not ventilate naturally without a shaft. The hotter air also accumulates in the roof of the attic at the ridge, and indicates the proper place for shaft openings.

The notes taken on 26th February, on which day only the shaft was entirely closed, show that there was hardly a perceptible draft either way and that *both ends* of the tunnel were clear, while all the rest of the tunnel was either *smoky* or *very* smoky. This also shows, as stated on page 365, that *under such circumstances* only so much of the tunnel as lies horizontally below the roof of the portals will ventilate naturally without a shaft.

The observations alluded to show in a majority of cases, when the draft is through the tunnel either way, that the difference of temperature between the ingoing end and the central shaft is greater than the difference of temperature between the outgoing and the central shaft.

Taking all the cases, including the exceptions, and we have an average of temperature difference between the ingoing ends and the central shaft of above 19°, and between the outgoing ends and the central shaft of about 14° or a rise of 5° between the entrance of the air and its departure, both being by the portals.

The average temperature at the ends was $\frac{5}{10}$ degrees, ranging between 15° and 50°, and at the central shaft $53\frac{3}{10}$°, ranging between 51° and 62°.

It will be noticed that the temperature at the ends average $4\frac{1}{2}$° above freezing, so that there would have been no danger from frost with the fullest ventilation, except in a few cases.

With the above difference of temperature of 19° between the ingoing end and the central shaft, what rate of ventilation should we expect with tunnel and shaft fully open, when a mine has two shafts of equal area, with tops and bottoms at same elevation? As soon as they are connected at the base, one will certainly become an upcast and the other a downcast; but the determining causes are so delicate, perhaps accidental, that no one can foretell which will become the upcast.

But in the case assumed, there can probably be no question but that the central shaft will become an upcast, while the exterior surface of the mountain becomes a downcast. It would be an interesting question, as to how much this downcast would be modified or affected by the mountain summits interfering between the surface openings and the shaft, and the two ends.

But the *speed* of ventilation is a more uncertain and delicate question than that of *direction*.

The central shaft, as it is stated on page 365, has an area of $318\frac{086}{1000}$ square feet. Its depth from ground to grade is $1018\frac{48}{100}$ feet, to which, perhaps, we are entitled to add the elevation of grade at shaft above that at portals, which is about sixty feet more.

The volume of air at 32° is doubled by an addition of 491° of heat. The area of a column of air being constant, its height will increase in the same ratio. How much will a column of air $1018\frac{48}{100}$

feet high be increased in height by the addition of 19° Fah., its original temperature before entering the tunnel being say $36\frac{5}{10}°$.

$491°+4.5 : 491°+23.5° : : 1018\frac{48}{100} : 1057.53$ Ans.

Logm. 495.5 7.3049563 by Rankine's rules and tables.
" 514.5 2.7113854 page 249. $V=29\sqrt{h}$. tables.
" 1018.48 3.0079525 $V=8.024\sqrt{h}$.

Th. = 1057.53 3.0242942 $8.024\sqrt{39.05}$ = velocity per second in feet =
1018.48
Logm. $39.05=h^3$ 1.5916210 50^{143} ft. per second =
0.7958105 3008^{52} " minute.
Logm. 8024 0.9043909 in the shaft.
50.142 1.7002014

Area of tunnel, 464 square feet: of shaft, 318,086 : : velocity, 3008.52 : 2062.43 ft. velocity in tunnel per minute.

Log. 464.		7.3334820	Log. 25081		4.3993448
" 318.086		2.5025446	" 2062.43		3.3143795
				min.	
" 3008.52		3.4783529	" 12.161		1.0849653
2062.43		3.3143795			

This speed of ventilation, which at the hurricane rate of 153 miles per hour, would clear the whole tunnel in $12\frac{1}{2}$ minutes, or about 118 times per day of twenty-four hours.

Just what influence, or rather the amount of it, the hot gases from locomotives will have, it is difficult to say, but the probability is, that it will have the effect to prolong the winter and shorten the summer conditions. From Rankine's Rules and Tables, page 115:

Log. 29.922 inches of mercury.....		1.4759906
" 14.7 lbs. atmosphere per sq. in.		1.1673173
2.0355		0.3086733
" 120 common pressure in engine		2.0791812
" 244.26 inches of mercury.....		2.3878545

From useful information (Ramopo) page 287, we find that $\sqrt[6]{244.26}\times 177 - 100$ = temperature of steam at 120 lbs.

Log. 244.26.....	$^6/$	2.3878545
		0.3979754
Log. 177		2.2479733
442.53		2.6459487

$\frac{100}{342.53}$ temperature of steam at 120 lbs. pressure.

From Ramopo, page 174, firegrate surface in engine, say 15 sq. ft.

From Rankine's "Steam Eng.," page 285, lbs. coal per hour per sq. ft. of grate, 40 sq. ft.

From "Rankine's Steam Engineering," page 286, cw. ft. of air per pound of fuel at 572°, 471 square feet.

Then cubic feet of air discharged from smoke-stack per hour 282,600, at a temperature between 342° and 600°, without allowance for its expansion, which about double it.

Suppose passage occupy 20 minutes, then a locomotive in one passage discharges, without allowance for expansion, $\frac{282600}{3} = 94200$ cwt. feet., which would fill a length of tunnel of $211\frac{64}{100}$ feet, as the passage of about 118 locomotives would fill the entire tunnel once full of hot air or gases, at a temperature of say 572°.

This shows a very considerable influence on the part of locomotives in producing a difference in temperature, and consequently an increase in draft.

There can be no question, whether considered in the light of theory, or from observation in the tunnel itself, that, when the air in the tunnel is warmer, and consequently lighter, then the exterior air, which is the usual winter condition, that the air from the tunnel will rise through the central shaft, and flow into the tunnel from one or both ends—depending on the direction and force of the wind exterior to the tunnel; and also that when the air of the tunnel and shaft is colder than the exterior air, which is the normal summer state, the air from the shaft will descend into the tunnel, and with that in the tunnel, run out at one or both ends, as water out of a pipe. Perhaps the change between *day* and *night* may make diurnal summers and winters, during the warmer months.

It should be said of the calculations, on this page, where it is shown that with a difference in outer and inner temperature of 19°, the entire tunnel will be ventilated in about twelve minutes; that

with this speed in the current of air, it *will not be possible* to maintain this difference of temperature, and as a consequence, this speed of ventilation.

Experiments with an open shaft *have not been made*, and therefore we do not know what influence the tunnel will have in heating or cooling the air-current at different velocities, nor what influence locomotives will have in addition.

It is plain that the swifter the current, the less the heating or cooling power of the tunnel and locomotives, because the current is exposed to their influences for a shorter time. They are mutually dependent, and the range of temperature will be between say zero outside and 50° within in winter, and 100° outside and 50° within in summer, and an equilibrium of temperature and velocities will be established somewhere between.

Could a difference of temperature of 50° be maintained, the volocity resulting would ventilate the whole tunnel in about seven minutes. This condition is not possible in the nature of things, but it illustrates the influence of differences of temperature in creating currents. Let us suppose that a difference of temperature of 5° between external and internal air can be maintained usually, and confined to that limit by the use of portal gates, then we have the following results:

Assuming the internal air at 50°, and the external at 45°, then $491+13 : 491+18 :: 1018\frac{42}{100} :: 1028\frac{58}{100}$, and equal $1028.58-1018.48=10.10$ feet.

Log. 504	7.2975695	$8.024\sqrt{10.10}=25.507$	
" 509	2.7067178		
" 1018.48	3.0079525	Velocity $=25\frac{101}{100}$ ft. per second.	
1028.58	3.0122368	Velocity $=1530\frac{42}{100}$ ft. per minute.	
Log. 10.10^2	1.0043214	in the shaft.	
	0.5021607		
Log. 8.024	0.9043909		
25.507	1.4065516		

Area of tunnel, 464 sq. ft.; area of shaft, 318.086 : : 1530.42; velocity in shaft, 1049.55 ft. per min. velocity in tunnel.

Log. 464 cos.	7.3334820	Log. 25081	4.3993438
" 318.086	2.5025446	" 1049.15	3.0208373
" 1530.42	3.1848107	2.3906 min.	1.3785075
1049.15	3.0208373		

This speed of current will clear the tunnel entirely once in about 24 minutes, or 60 times in the 24 hours.

If the shaft should be closed by masonry, a man-hole of three feet diameter be left open, and the flues neglected, as has been proposed, we should have the following state of things, with the supposed difference of temperature of 5°.

Velocity in the shaft being as before $1530\frac{42}{100}$ feet per minute, then area tunnel 464; area man-hole 7.0686 : : velocity shaft 1530.42; velocity tunnel 23.314.

Log. 464 cos.	7.3334820	Log. 25081		4.3993448
" 7.0686	0.8493334	" 23.314		1.3676261
			min.	
" 15.3042	3.1848107	$60)1075.77$		3.0317187
23.314	1.3676261	17 hrs., $55\frac{77}{100}$ mins.		

That is, the tunnel would be ventilated once through in about every 18 hours.

The ventilation of the tunnel will not be beneficially, but rather prejudicially, effected by the *movement* of trains. They will not push a column of air before them through the entire tunnel, nor up the shaft. The Hoosac Tunnel is too long and its open area too large to permit this. The cross-section of a moving train will not exceed 110 square feet, while the tunnel section averages 464 or more than four times as much. The air temporarily pushed before a train, will return overhead and along sides in an eddy, to supply the vacuum left behind.

In an article in the *Engineer*, vol. 39, page 30, January 8th, 1875, we find such an experience in the Metropolitan Railway of London. The tunnel is 4,200 feet, or about three-fourth mile long. The air returning from before the trains, comes again in an eddy to the rear of the train. To prevent this, the engineer puts up screens and doors overhead and at the sides, but such a course is inapplicable to so extensive a work as the Hoosac.

In the calculations of velocity of currents of air herein made, allowance has been made for the vein of contraction and for friction only in the case of the flues, which are made each 180 square feet, while one-half of the shaft area is 159 square feet. If the contracted area be taken at ninety-three per cent. of the full area of opening, we have 93×180 :

167.10 square feet, an excess still of about eight feet, and probably sufficient for friction at slow velocities.

There will be times between summer and winter, and between day and night, when temperatures in and out of the tunnel, will approximate to each other. Then there will be little *natural* ventilation. It will then, however, be true that the hotter air of the tunnel and from engines, will accumulate in the apex of the attic. It may then become possible, and in all probability *necessary* to divide the entire shaft by a transverse partition, so as to obtain a ventilation by means of two separate flues, the inside air of the roof ascending in the one, and the outer air descending by the other. In Ure's Dictionary, vol. 2, page 224, it is stated that with two such flues "it is generally found that a current of air does take place (it may almost be said *always* takes place) without any other means being employed."

The necessity for doing this, if in the hands of proper observers, will be experimentally proved or disproved, and until it be *proved*, the partition need not be put in. Nothing should *now* be done however, which will *prevent* the easy accomplishment of this end, and there is nothing in the design prepared on page 25, which will forbid the proper carrying out of the double flue plan.

There is a *chance* that with a heavy traffic *artificial* aids to ventilation will be required in addition.

In the abstract from a paper read by Mr. Marrison, to the Institute of Civil Engineers in London, to be found in the *Engineer* of January 21st, 1876, and referred to on page 363 hereof, it is stated that should the trains be increased from the present number of sixteen in the Mont Cenis Tunnel up to twenty, that *mechanical* ventilation would probably be an *absolute necessity*.

The article further shows, that with a shaft at the center there would be a great gain in convenience and power.

Mr. Marrison concludes:

1st. That long tunnels *without shaft* must be ventilated by fans at one end.

2d. That with a given amount of traffic, the power required to ventilate, varies as the fourth power of the length.

If this second position be true, it will be 509 times as difficult to ventilate the Hoosac 4.75 miles long without a shaft, as a tunnel one mile long.

But if we treat the shaft as an open end, than which if kept open, it will probably be better, we have only one-half the length, or 2.375 miles, and under the second position, the Hoosac would then be about thirty-two times more difficult to ventilate than a tunnel one mile long.

Log.	2.375	0.3756636
		4
"	31.81	1.5026544

The comparison between the tunnel 4.75 miles long and that of one-half the length, in which the one requires 509 times, and the other only thirty-two times the power to ventilate over a tunnel one mile long, illustrates, *very forcibly*, the importance of the fullest liberty to use the central shaft.

There are three prominent methods of *artificial aids* to ventilation:

By fires in a shaft or chimney;
By forcing fans, or pumps;
By exhausting fans or pumps.

When coal is cheap, a fire at the bottom of *one* flue of a double shaft would be perhaps the best method. This plan would, however, not be available if the shaft is to be maintained as a single flue, for at times the draft should be down the shaft.

The plan of forcing fans is not thought well of in practice. It has been said of air, that it is like a string which can easier be pulled through a tube, than pushed through. In Ure's Dictionary. vol. 2, page 416, it is said that "a very slight circulation of air can be effected by propulsion in comparison of what may be done by exhaustion."

This being the case, the best form of mechanical aid to ventilation, should it ever be wanted, will be by exhausting fans set at the top of the central shaft.

Fans to be efficient for such a purpose should be very large. In the old country, they are built thirty and forty feet in diameter. The *whole area* of the shaft must be reserved for their use if ever introduced.

Mr. Henry S. Drinker, who is prepar-

ing a work on American Tunnelling, suggests the application of *wind power* to driving fans.

Benjamin Gibbons, Ure's Dictionary, page 1017, in regard to mining ventilation, writes: "We should avail ourselves, as far as possible, of the *natural powers* that are at our command. But cases may arise when other auxiliaries may be temporarily required, although in the author's opinion these cases may be reduced to a very few."

THE RAILWAYS OF THE WORLD.

From "The Engineer."

THE title of this article deserves a word of explanation. The subject denoted by it is enormous. How is it possible to say anything concerning it which is worth saying within the limits at our disposal? We venture to hope that before we have done we shall be able to justify our selection. At the end of the year 1873, the Honorable Mr. Gillies, Australian Commissioner of Railways, informed Mr. T. Higinbotham, M.I.C.E., Engineer-in-Chief of Victorian Railways, that his Government considered it desirable that he should visit Europe and America, inspect the railways of those countries, and prepare a report concerning them for the information of the Australian Government. Accordingly Mr. Higinbotham left Melbourne on the 10th of March, 1874, and visited America, Great Britain, Europe, and returning by way of India, inspected most of the great lines in that country. He arrived in Melbourne again on the 11th of January, 1876, having thus been absent a year and ten months. During his tour Mr. Higinbotham met everywhere with the utmost courtesy. He was freely supplied with information, and he learned a great deal. This information he has embodied in a very terse and ably-written report of seventy-two large pages, and it is with this report on the railways of the world that we now propose to deal. It will be understood at the outset that there are multitudes of questions connected with the working of railways with which Mr. Higinbotham did not concern himself. The paramount object he had in view was to collect data which would be useful in Australia. But he has done more than this, for he has contrived to put into his report a summary description of the railways of the world which contains a very considerable mass of general information, rendering the paper one of the most valuable contributions to the literature of railways ever penned. We do not propose here to go fully into Mr. Higinbotham's report; for the present we shall content ourselves with dealing with some of the facts he places before his readers. Mr. Higinbotham went first to the United States. He reached San Francisco on the 15th of April, 1874, and his first stopping place was Sacramento city, where he inspected the workshops of the Central Pacific line. Of this road at the time there were 1600 miles open; the main line from San Francisco to Ogden, where the Union Pacific line begins, being 882 miles long. Mr. Higinbotham deals tenderly with the shortcomings of this railway, while he describes its general characteristics very succinctly. We have not space to reproduce in full his description of the rolling stock; it must suffice to say that the engines are all fitted with bogies, or "track feelers," as they are expressively termed, which carry from thirty to forty per cent. of the whole weight of the engine. The passenger engines weigh with steam up about thirty-three tons; they have seventeen inch cylinders, twenty-four inch stroke, and four coupled drivers four feet eight inches to five feet in diameter; the tenders weigh full twenty-five tons. The heaviest gradient worked is one in fifty over the Sierra Nevada. Three engines are required to take up a train of nine or ten passenger cars. These cars each weigh about thirteen tons, of which the two bogies represent about 5.5 tons. They seat fifty-two, and when the car is full the dead weight per passenger is 754 lbs. Mr. Higinbotham gives similar

particulars of all the lines with which he deals. Sometimes the details are better filled in, sometimes the information is more meagre; but, on the whole, he supplies just what is wanted. We shall not attempt to follow him step by step. It will be of more interest to our readers to learn something concerning the opinions of a competent engineer, who has had innumerable opportunities of acquiring information. Mr. Higinbotham has a good deal to say, for example, concerning a much-vexed question—namely, the relative merits of English and American locomotives, and we shall confine our attention to this branch of the subject for the moment.

It has often been stated in this country, as well as in the United States, that the American locomotive is cheaper to build, to maintain, and to work than the English engine, and the statement has been disputed as often as it has been made. Mr. Higinbotham reports that all the evidence he has obtained goes to show that on bad roads, such as exist in the United States, English locomotives could not be used at all; from which it appears that the American engine has a distinct vocation, and that it is vain to draw comparisons between the two types of machine. In writing about the Pacific Railroad, for example, Mr. Higinbotham says:—"I had opportunities of speaking to several drivers, Englishmen, who had driven in England; they all preferred the American to the English engine, which, they said, could not keep on such roads as are commonly to be found in the Western States." This may be taken as unprejudiced testimony: "The road was rough, and the speed at times down falling gradients very great, but the drivers appeared to have perfect confidence that their engines would not leave the road. The bogie they consider the great source of safety." It may be said that as these men had not tried English engines on the road they could not speak from experience. In his account of the Grand Trunk Railway of Canada, however, Mr. Higinbotham subsequently says:—"The whole of the engines and rolling stock first put on the line were built in England, but the engines were constantly getting off the road, and engines and cars of every class are now of the same kind as those in the States." Further on we read:—"The bogie truck and cast iron wheels are two of the most important features of American engines and rolling stock, and both of these appear to me to have been adopted in consequence of the very imperfect permanent way and the great severity of the climate in the winter, when the road bed, which is frequently formed of earth only, is exposed alternately to intense frost and sudden thaws, which completely distort the track. Only bogie engines and rolling stock could live on such roads, as was proved in the case of the Grand Trunk Railway of Canada, where, neglecting the experience that had been gained in the States, and relying also no doubt on a better and more carefully laid road than was to be found there, English engines and rolling stock were tried, but had to be abandoned, and the American type adopted. Very recently the same mistake was made on the narrow gauge railroads in Canada, and with the same result." We cannot ignore testimony of this kind, and it may be taken as conclusively settled that bogie engines, and American bogie engines in particular, are less likely to be thrown off a road than English engines. This result is due no doubt to two causes. The first is the comparative flexibility of the American engine, the whole wheel base of which can twist itself about so as to follow the vertical or horizontal contortions of a road with great ease; while the second is that the leading wheels of the American engine bogie are left very much to themselves, and are but little exposed to the influence of the lateral motion of the engine. It was once proposed to construct an engine with six carrying wheels, the leading wheels to be set almost precisely like the fore carriage of an omnibus, while two smaller wheels, which supported none of the weight of the engine, were to run five feet or six feet in front, the end of the axle being carried in radius rods, if we may so call them, coupled to the ends of the true leading axle of the engine. The apparently idle wheels would, it was contended, never leave the road, for they would be spared all side shocks, and they would continually guide the true leading wheels, steady them, and so prevent them from leaving the rails, taking them nicely into and out of

curves, and radiating them to the best advantage. There is reason to believe that the leading wheels of the American bogie in a great measure answer this purpose, and so help to keep the engine on the road. Now, it is true that English engines on English roads very seldom run off, in spite of their long and comparatively rigid wheel base; but this results not from the merits of the engine, but from the excellent qualities of the road; and it is worth considering whether an American engine, which is capable of running well on a road which sets an English locomotive at defiance, might not be found to run more lightly, cheaply, and with less practical resisttance, and less wear and tear of the track, than an English engine. In this country bogies are used almost entirely in order to distribute weight, and with little or no regard for facilitating the motion of an engine or preventing the chance of derailment; and a considerable number of bogie engines have been so badly designed that the bogie itself has suffered in reputation. But there are a great many very well designed bogie engines running in Great Britain, and it would be very interesting to know what the running expenses of such bogie engines as those on the North British Railway, for example, are as compared with the working cost of Midland or London and North-Western engines of about the same weight, and with six wheels only.

It might thus be ascertained whether a still further assimilation to American practice is or is not desirable. We have no intention of lauding American locomotives and disparaging our own; but, on the other hand, English engineers would not act wisely if they suffered a silly question of prejudice to stand in the way and shut out facts of the kind supplied by Mr. Higinbotham. Whatever this gentleman may think of American engines, it is certain that he holds the American railway car in very low estimation. He points out that the bogie system is only applicable to carriages of large dimensions, at least as bogies are made in the United States, where they weigh from five and a-half to nine tons. As regards comfort, he holds that the American car cannot compare with the English carriage, more especially for long journeys; and it does not appear that anything whatever would be gained, while a great deal would be lost, by the adoption of such cars in this country. It must be understood that the ordinary American car, to which alone Mr. Higinbotham is referring, has little or nothing in common with the Pullman palace and sleeping cars, which are admirably adapted to long journeys. We have referred so frequently to cast iron wheels in the course of this article that we ought perhaps to say something more concerning them here, but we prefer to reserve our consideration of the information Mr. Higinbotham supplies for another occasion.

Before taking leave of the American railroads, Mr. Higinbotham sums up most of the information which he has obtained. Indeed, his general rule in writing appears to have been first to explain how he acquired his information; and secondly, to lay that information before his readers. In doing this, we find that he constantly combats Victorian prejudices, or compares Victorian practice with that of other countries. Thus it appears that when strict economy was urged some years since on the Victorian Government in the construction of their railroads, it was repeatedly asserted that in the United States there were no platforms at the railway stations, whereas platforms are to be found at every station of any importance in the States. In the West they are seldom raised much above the ground, but a far larger area is allowed than is common in England. The fact that no railway signals as we understand them are used on American railroads will be new to many English engineers. The great majority of the lines being single, are worked by telegraph, and at stations by hand signals. An officer who is known as a "train dispatcher" has entire control of the running of the trains on a section of from 100 to 200 miles of road. He regulates their working by means of a diagram on which is shown the position of every train at any given moment on the section which he controls, the information of course being collected into his head office by telegraph. If a breakdown occurs, he is able to control and readjust the traffic by means of his diagram. The staff system, it seems, is never used either in the States or Canada on single

lines. There is a great deal to be urged in favor of a system of this kind, which places before the controlling mind a complete picture in little of the position of the various trains in a given district almost from minute to minute; but attempts to work on a similar system in this country have not succeeded very well, apparently because it is almost impossible to induce the station-masters and other officials to communicate freely with the train dispatcher, who, it is obvious, becomes powerless the moment he is deprived of information concerning the whereabouts of a train.

For the moment we take our leave of Mr. Higinbotham's report, giving in conclusion the following expression of his opinion, which we commend specially to the attention of those gentlemen connected with the iron and steel trade who expect to find in the United States, ere long, a good market for their productions. "It is admitted in the United States that, for the present, railway construction has reached its limits, and that the progress in the future must, necessarily, be very slow and gradual as compared with the past. Among the Rocky Mountains and in similar districts, there will possibly be extensions of mineral lines on a gauge of three feet or three feet six inches; but the construction of railways for general traffic is at an end for the present, and possibly for some time to come." An expression of opinion from a man who possessed Mr. Higinbotham's unrivalled opportunities of acquiring accurate information possesses unusual value.

THE CHAIN TAG, "IROQUOIS," ON THE RIVER ST. LAWRENCE.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE Canadian Government for the last five years have been deeply engaged with the improvements of the St. Lawrence navigation. The Welland Canal has been thoroughly surveyed, and the works are now well under way. In the case of the St. Lawrence canals proper, they have all been carefully surveyed. The Lachine and Cornwall are now let and the works advancing. But with regard to that part of the St. Lawrence navigation, known as the Williamsburg Canals, nothing has been done, except the surveys, which have been very thorough. Those canals are only used by ascending vessels, descending ones go outside. The river here will not accommodate anything drawing over 9′ at low water, but it is intended by the D.P.W. to blast out a channel through the rock in the Galoups Rapids (Rapide Aux Galoups), and thus accommodate descending vessels of an increased draught; at the same time save the expense of enlarging the canals for passing vessels.

To do this blasting, the department have placed a Chain Tag in the rapids, which, if successful in the blasting operations, and in clearing a channel of the desired depth, will be tried as a tow-boat to haul vessels up the rapids. If successful in this, the canals will not require any enlargement, as all navigation will be done in the river; thus saving time and money lost in canaling.

The chain tag was placed in position, August 24, 1876. The chain at the upper end is made fast to Beldon Island, and laid on the bed of the river for one mile below. This mile is only for experimenting with; in event of success it will be lengthened five or six miles.

The chain is of $1\frac{1}{4}''$ iron, weighing $14\frac{1}{2}$ lbs. to the foot run, and was made in England. The tag is very ugly, but very powerful. The engine is of 345 H. P., placed horizontally on two oak keelsons. She has a pair of 22″ cylinders of 60″ stroke, and the pistons connect directly by a simple motion to the "drums," which are two in number, 7′.7″ in circumference each, and take five turns of the chain. The drums are plain with the exception of slots for the inner segments of the upright links of the chain which is held by friction only.

It is worthy of note here, that the chains of the chain tag in the Hochelaga

current at Montreal is found to *bind* too tightly, so that it has to be paid out several inches every day. The haul here is very short and straight.

The chain tag at the Galoup is built for blasting, and has six "speeds" or legs of rock elm 14″ square, and 36′ long, which are lowered down through watertight "speed wells," built in the vessel. Four of these are in the bow, and two in the stern. All of them are used to anchor, but the bow four are used for drilling as well. The drilling is done in a boiler-plate tube at the back of the "speed."

At first the drilling was tried in open water, but the eddies were such that the drills would not strike twice in the same place, notwithstanding the "guide" place was within 12″ of the rock. The rock is about 11′ or 12′ below the surface of the water, and it is intended to blast it away to 16′ below low water.

The rock is hard, crystaline limestone, and is found to dull the drills very quickly.

Nothing of any consequence was done last season as the tag was not finished in time—even after she was placed in position the alterations and improvements occupied some weeks. The only conclusions come to are:—First, It is possible to move her up and down on her chain in the rapid. Second, That it is possible to anchor her anywhere, if her "speeds" or legs will reach the bed of the river (any amount of weight can be brought to bear on the "speeds" by lifting the tag on them). Third, It is possible to drill behind the "speeds" anywhere.

Putting in the charges of glycerine, and setting them off, has not been tried yet. And as a tow-boat she has not been tried. Indeed, it is thought she will not answer for towing, except when the "haul" is straight.

We believe the Department is very much disappointed in her in every way, and it is intended to give her work out by contract; but any way, it is intended during this session of Parliament to decide what is to be done with her.

The experiments with this chain tag have not been full enough (and in this respect are like many other Government experiments) to give a decided opinion concerning her.

REPORTS OF ENGINEERING SOCIETIES

KING'S COLLEGE ENGINEERING SOCIETY.—At a meeting of this Society on February 9th, a paper on the properties of dynamite was read by Mr. J. C. Mackay, who is now engaged on the works of the tunnel which is in course of construction at Dowlais, Glamorganshire. Mr. Mackay directed attention to the need of a highly disruptive explosive, and at the same time of one available to carry on the arduous operation of the miner with safety and economy. He pointed out that such an explosive should not be affected by damp, should not require large borings, involving expensive work, should not generate an accumulation of smoke, not be too easily ignited, and should possess facility of transport and storage. He showed that dynamite, in careful hands, fulfilled to a great extent all these conditions, and was specially adapted for tunnelling through hard strata of rock. It was not injured by moisture, a quality which no other explosive possessed; it would only explode by detonation, and was not explosive by ignition. It presented an economy in labor which, as compared with other explosives, was in the ratio of 4 to 3. The result of the comparative cost of experiments in Clifton tunnel, for driving fifteen lineal yards, he gave as follows: For powder £29 10s.; for gun-cotton £18 2s.; for dynamite £16 10s. Mr. Mackay also referred to the results obtained by experiments under his own observation at Dowlais, and pointed out, that the use of dynamite must be regulated by the character of the rock, and that though most economical when applied to harder rocks, yet it was found to be less useful in the case of coal and the softer friable rocks. In conclusion, Mr. Mackay referred to the result of the explosions at Hell Gate, as showing the extreme value of dynamite as a submarine explosive. In the discussion which followed, observations were offered by Mr. Douglas, the President, Mr. Alliman, Mr. Compton, and Mr. A. Percy Guinness, the honorary secretary, and a vote of thanks was passed to Mr. Mackay for his interesting paper.—At the meeting of this Society on the 2d ult., a paper on "The Art of Moulding" was read by Mr. E. W. Anderson, A.K.C., hon. member of the Society, and son of one of the members of the well known firm of Eastons and Anderson. The author explained the different methods which are practically used in the art of moulding, and directed attention to the following general rules, which should guide the moulder in the running in of the metal in moulding: 1. Choose if possible the thickest part of the casting for the runner. 2. If the casting is deep, run in the metal at the bottom. 3. Where the casting has a flange in the form of a pipe, it is generally preferred to run the metal in at the flange; but this case is subject to Rule 4. 4. Where the casting is thin and has many branches, or when it is of great length, it is advisable to run in the metal in the centre. 5. Care should be taken to choose a place in the mould so that the metal will have no tendency to wash any part away in its first rush. 6. (This rule may be called a continuation of No. 5.) The metal should not be allowed to

fall from any height upon a weak part of the mould, or it will cause the liability to break down portions of the mould. Mr. Anderson produced several patterns in illustration of the subject to which he referred, and the tools which are used in actual work, which he presented to the museum of the college. The proceedings terminated with a vote of thanks to Mr. Anderson for his interesting paper.

IRON AND STEEL NOTES.

THE NEW NOMENCLATURE IN GERMANY.—We are indebted to the courtesy of James M. Swank, Esq., Secretary of the American Iron and Steel Association, for the following copy of a letter from Dr. Hermann Wedding, of Berlin, on the new nomenclature in Germany:

BERLIN, Germany, Jan. 5, 1877.

To the Secretary of the American Iron and Steel Association:

SIR,—Every system has its defects; hence every nomenclature also. The settlement of a system can therefore only be founded upon a compromise, and a system so made is the better the more its nomenclature covers the principal abstracts, and leaves the uncertainties to the lowest divisions. That was my opinion as I put my name under the International Committee's report.

Our ancestors made by the ways of the direct process, or of the charcoal finery, two kinds of forgeable iron, which always differed essentially in the amount of carbon, had, in consequence, essentially different physical properties, and were used for essentially different purposes. The intermediate kinds of iron were not in use, and were therefore not purposely produced. The puddling process originated at first an intermediate product, the so-called "fine-grained iron."

All these products were obtained in a pasty state. Now comes the Bessemer process and produces a fluid, forgeable metal, and several adjoining processes do the same. Fluid irons are now easily made of different amounts of carbon, and every intermediate product is blown without difficulty. From this time the property of hardening no more stands in the foreground; it goes back, and the properties which are a consequence of the fluid state take the leading place. At the same time arises the necessity of a new nomenclature.

The nomenclature of the International Committee is known. Let me show the system:

- Iron.
  - Forgeable iron. I.
    - Forgeable; difficult to melt:
      - Obtained in a fluid state: A. Ingot iron.
        - Hardening: 1. Ingot steel.
        - Not hardening: 2. Ingot iron.
      - Obtained in a non-fluid state: B. Weld iron.
        - Hardening: 3. Weld steel.
        - Not hardening: 4. Weld iron.
  - Pig iron. II.
    - Not forgeable; easy to melt:
      - With amorphic carbon only: C. White pig.
      - With graphite: D. Gray pig.

There may be added, called as before: *Remelted* pig iron—cast iron; *remelted* steel of every description—cast steel. Here the division following the fluid or non fluid state keeps the principal place; the hardening only the second. Therefore, all the members of the Committee, differing in some points (Mr. Akerman and myself wishing a more detailed subdivision), agreed to this compromise without hesitation.

It is asserted that such a compromise, comprising the languages of the principal iron-producing peoples, has not been necessary, and that each country may keep its own expressions without caring about others. But how can that be good and useful? I think every one, Protectionist or Free Trader, must be a friend of clear and distinct definitions, whether in treaties of commerce or in simple tariffs. And even besides this international question, is it not necessary for the public trade to have accurate definitions for every kind of ware, giving no room for intentional or unintentional deceptions?

By the new nomenclature accepted and introduced, every tradesman and every customs' officer who knows nothing about iron metallurgy easily distinguish: I and II by the forgeability; C and D by the color; A and B by the fracture—if necessary, by an easily made etching with acid; 1, 2, 3 and 4 by the hardening. Also, those ought to be satisfied who desire to examine the chemical properties, viz.:

- Iron.
  - I. contains O 2, 3°° C.
    - 2 and 4; O-O, 6°° C.
    - 1 and 3; O, 6 2, 3°° C.
  - II. contains 2, 3-5°° C.
    - Amorphic C.
    - Graphitic and amorphic.

But it is not necessary to make analysis, and to dwell upon differences growing from the presence of other matters, which would be the case if you classify according to the amount of carbon.

After having read all that has appeared in your *Bulletin* and *The Engineering and Mining Journal*, I have no hope that our nomenclature will be accepted in America. But let me tell you, we are happier! It may be the interior value of the nomenclature, or it may be the authority of the names of the international committee, that has accomplished the result; but what long discussion could not do is done! The nomenclature *is* accepted in Germany.

The professors of iron metallurgy in Germany, Austria and Hungary have accepted it; the most widely-circulated journal of mining and metallurgy and the official journal of the ministry of trade and commerce have accepted it; the Association for the Progress of Industry at Berlin and the Association of Miners and Smelters at Karntnen, have accepted it. It will be introduced very probably in all official statistics. So we are certain that at least our rising generation will unanimously use the nomenclature proposed by the international committee.

Well, you Americans will of course do what you think best; but please accept our German thanks, added to many thanks for the kind and amiable reception given us in your wonderful

country, that you again have been the best help to a uniform nomenclature of iron in Germany. Very truly,

Dr. HERMANN WEDDING.

RAILWAY NOTES.

FRACTURE OF RAILWAY TIRES.—At a meeting of the Institution of Civil Engineers, in November, a paper was read on "The Fracture of Railway Tires," by Mr. W. W. Beaumont, of which the following is an abstract:

It was stated that between the years 1847 and 1874, eighty accidents from broken tires, attended by serious results, had been reported upon by the officers of the Board of Trade. The total number of tires fractured was not known, as previous to 1872 the railway companies made no return of such accidents; but, since 1847, tire fractures had resulted in the loss of seventy-four lives, and 236 cases of serious personal injury. So far as the author was aware, no satisfactory explanation had been given of the forces productive of fracture of tires of good material and workmanship; it was the object of this paper to suggest a cause for their origin. Some of the theories advanced to account for tire fracture were treated of, such as the strain due to shrinking tires on to wheel bodies, the reduction of the sectional area by rivet or bolt holes, and the alleged reduction of the strength of the tire by low temperatures in winter. These causes were considered to be inadequate to account for—(1) The fracture of a good tire; (2) the fracture of tires in several places simultaneously; (3) the fracture of tires through the solid body rather than through bolt or rivet holes; (4) breakages being few in number for a long period, and then occurring frequently; and (5) tires, generally of considerable age, running several thousand miles before flying to pieces.

For an explanation of these facts, the author appealed to internal differential molecular strains, generated in the material of tires, by extension and compression from their surface inwards, consequent upon their rolling at high velocities under heavy loads, along the hard, smooth and somewhat rigid permanent way. If a piece of flat stout plate metal was subjected, when cold, to long-continued light hammering, or rolling, on one of its surfaces, that surface would become compressed and elongated. The effect of thus altering the relative dimensions of the two surfaces of the originally flat plate, would be to make it assume the form of an umbo, with the convexity toward the rolled or hammered surface. In illustration of this, reference was made to the straightening of coping plates, or other plate castings, which had become bent in cooling, by lightly hammering the concave side and thus elongating that side. Another example was afforded in the curvature produced in tram plates, for instance, those on Westminster Bridge, by the extension and compression of the surface exposed to the rolling under the loaded wheels of vehicles. Similarly, film after film from the surface inwards of the material of a railway tire was compressed, until the thickness molecularly altered, induced internal differential strains sufficient to rupture the tire, or so nearly to effect it, that an unusually heavy impulse, or other extraneous force, was alone necessary for such a result. As these strains approached equality throughout the tire, the length, and therefore the number of pieces into which the tire would be broken, would be determined by the limit of stability and the coefficient of elasticity of the material. Absolute simultaneity of multiple fracture was not a necessary condition of such a result, as the precedence of one fracture would liberate the tire, so that the internal forces would be free to initiate fracture in as many places as might be necessary to expend the excess of the forces tending to rupture, over those of resistance to it. Of the reported fracture of tires, affixed by rivet, or screws, nearly one-third were fractured through the full section, and not at a bolt or rivet hole; thus indicating that those tires were either strongest at the reduced section, or that the internal forces tending to produce fracture were greater between than at the rivet or bolt-holes. In tires of good material and workmanship, fracture would be expected to take place rather between than through such holes; for at these the continuity of the material was broken, so that the tangential compression, produced in the outer portions of the tire by the impeded elongation of the material, was dissipated by an upward flow of the particles around such holes. This upward flow tended to produce a crater-like ridge, which was quickly worn off, so that the tire at these points was relieved of strain, the material that would have exerted it being carried away. Elastic wheels could only be considered as a palliative, for the tire had still to support a load, so that its surface would be subject to compression, although the mischief would not proceed so rapidly as with a nearly rigid wheel. The inertia of impact strain upon a rigid wheel would have to be overcome by the tire before it was relieved by the springs of the vehicle, whereas a good elastic wheel possessed in some degree the character of a spring, and in so far was without any such inertia. However good the material and workmanship of a railway tire, and in whatever manner affixed, it must gradually become unsafe, from other reasons than simple loss of thickness, for whether it was of steel or of iron, it was amenable to the production and accumulation of molecular strains. The great durability of American chilled wheels was probably owing to the extreme hardness of their running surfaces, and their consequent resistance to surface compression. Although the ultimate strength of a tire was probably not reduced by the bolt or rivet holes, the preferable method of fastening was unquestionably by continuous clips and grooves on both sides of the tire, so as to prevent the portions of a fractured tire from leaving the wheel. It was to this latter cause, rather than to simple fracture, that many lamentable accidents were to be ascribed.

RAILWAY-BRAKE EXPERIMENTS.—Important experiments with railway brakes commenced recently on the Edinburgh and Glasgow section of the North British Railway. Amongst the gentlemen present to assist in the experiments there were official representatives of railway companies in all parts of the kingdom.

The train, with which the first series of experiments was made, consisted of a locomotive engine and tender, eight carriages and two vans, the whole weight of which was about 155 tons, exclusive of probably fifty persons, who may be taken as averaging at least 12 stones, or 1¼ cwt. each; and the continuous brake system applied to the train, was that known as the Westinghouse automatic, which we previously described. All the wheels of the train were "braked," with the exception of those of the bogie on the front part of the engine, and those of the van in which the results of the experiments were observed and recorded; and the arrangements were of such a character that the brakes could be applied throughout the whole of the train, either from the engine or the recording van separately, or from both, at the same instant of time. The van alluded to, a spacious covered vehicle, capable of accommodating fully a score of people, was set apart for the use of the ingenious apparatus previously spoken of as the Westinghouse train-speed indicator. Messrs. Stirling, Haswell, Cowan, and Wright, with the assistance of Mr. Drummond, North British locomotive superintendent, were a committee for determining the experiments to be performed.

When everything was announced to be in readiness, the train left the Waverly Station, Edinburgh, at about a quarter past nine o'clock, on the run to Glasgow. On the westward run between Edinburgh and Cowlairs, there were eleven applications of the brakes when the train was running at speeds ranging from thirty up to sixty miles per hour, as shown by the speed indicator. As a rule, the experiments were not made at first with that amount of reliability that was desirable, and several of them were not taken into consideration in officially recording the results. Up to the time when the train had passed Linlithgow Station, it had never attained a speed of more than fifty-five miles per hour, and the greatest speed —sixty miles an hour—was attained when the train was passing Woodilee Asylum, Lenzie. One stoppage was effected in twenty-eight seconds, when the speed was fifty-four miles an hour; at another time a complete standstill was attained in twenty-nine and a half seconds, when there was a speed of sixty miles an hour on the train.

After stopping some time at Cowlairs, where the party was largely increased, shortly after eleven o'clock a return journey to Edinburgh was made, the recording van being the last vehicle in the train. In running what was called the "up express" no fewer than eighteen experiments were made. Between Cowlairs and Polmont, there were ten stoppages while the train was running at speeds varying from thirty to sixty miles per hour, the complete stop being secured in periods ranging from ten and a half seconds up to twenty-seven seconds. The eleventh stoppage was done in twenty-four and a half seconds, when the speed was at sixty miles an hour. A later stoppage, some distance eastward of Winchburgh, when a speed of forty miles an hour had been attained, was effected by applying the brakes from the van alone, and without any communication with the engine-driver. A dead stand was attained in thirty and a half seconds. In this case the engine was not, we believe, brought to bear in the stoppage of the train, and hence the longer time required to secure the stoppage. Another experiment of a similar sort was made when the speed was indicated at forty-five miles an hour, and complete rest was got in thirty-four and three-quarter seconds. Lastly, just as the train approached the Edinburgh Station, the brakes were applied when the speed was forty miles an hour, the application being made simultaneously in the van and on the engine. The stop was effected in nineteen and three-quarter seconds.

During the progress of the experiments, no cognizance was taken of the distance over which the train ran from the moment of applying the brake-blocks to the wheels and the complete stoppage; but from the results obtained, the distances will in due time be worked out from tables constructed for the purpose. Under ordinary circumstances a train-speed of sixty miles an hour should be perfectly overcome in about 300 yards, or 900 feet.

Experiments with Smith's vacuum brake will take place on another occasion.—*Iron*.

ENGINEERING STRUCTURES.

NEW ENGINEERING DEVICE.—A peculiar work has been completed by the Ches. & Ohio Canal Company to make a new outlet from the canal to the Potomac. The elevation to be overcome is forty feet, which under the old system would require eight locks and nearly an hour's time for the passage. The new arrangement is the employment of a large caisson, or tank, filled with water, into which the boat is floated, when the caisson, with its load of water and boat, is run down the incline on rails in less than six minutes. The weight of the loaded caisson is about 350 tons. The first expense of constructing locks and the constant expense of operating them and keeping them in repair is saved, as well as the time.—*Railway Review*.

NEW DEVICE FOR RAISING WATER.—M. Th. Foucalt has recently produced a new apparatus for raising water by means of ammoniacal gas. The machine depends for its operation on the facts that water at 15 deg. Cent. absorbs 743 times its volume of ammoniacal gas, and gives it off again at 60 deg. Cent.; that at 100 deg. Cent., the tension of the vapor is seven and a half atmospheres; that petroleum and ammoniacal gas are without action upon each other; and that the same is true of petroleum and water. The apparatus consists substantially of a heater, which is partially filled with a strong aqueous solution of ammoniacal gas.

This heater is connected by pipe with the upper part of a closed reservoir, the lower part of the reservoir being connected by means of pipe and suitable valves with the steam or well from which, and the tank to which, water is to be raised. The reservoir contains a small quantity of petroleum, which forms a thin stratum on the surface of the water, and serves to keep the ammoniacal gas from contact with it, and, as the inventor expresses it, forms a fluid piston. The operation is as follows: Supposing the reservoir full of water, the temperature of the heater is raised by suitable means, ammoniacal gas is given off, and passes over into the upper part of the reservoir, the stratum of petroleum preventing its being absorbed by the water there. A pressure is thus created in the reservoir, which forces the water there out and up to the tank to be filled. When all the water has been forced out of the reservoir, the heater is cooled by removing the fire and allowing a jet of water from the tank to play on it. The water in the heater, as it cools, re-absorbs the ammoniacal gas from the reservoir, and thus creates a vacuum, which the water from the stream or well rushes up to fill, and thus refills the reservoir. The heater is then heated, and so on, as before. The inventor claims that the consumption of fuel is almost insignificant as compared to that of a steam-pump of the same capacity. The author also describes a modification of his apparatus adapted to be run by the heat of the sun, in which case the only expense is that of wear and tear, which is small, there being no moving parts.

EGERTON'S TIDAL PIER.—When Mr. Egerton brought out the ship built on pontoons, which we noticed some time ago, he was met, among other objections, with the difficulty of providing a sea pier which would permit of trains being run at the varying levels of the tide direct on board. This objection he set himself at once to meet, and last week he exhibited to various engineers and other persons interested in the matter a model of a very ingenious pier, built in sections, and apparently very well contrived for its special object. The connection between the shore and the floating pier, instead of being supported by continuous girders, is made up of a number of short sections, supported by pontoons at the points of connection. The pontoons are set in guides, and in the case of seaports a breakwater may be added to protect them against excessive motion by the waves. The actual support which connects each pontoon with the roadway is a kind of gigantic "lazy tongs," which "gives" to exactly the extent needed. In the model, high and low water was produced by adding and drawing off water as needed; and, however great the change in level, a perfectly straight though not rigid road is maintained. A railway train could thus, the inventor claims, be run on to a vessel, be carried across an arm of the sea—or the Channel itself—and be run on shore again, so as to travel from London to Paris without change of carriage. But lest this be considered too great a task, it should be mentioned that this pier is equally adapted for smaller achievements. It is suggested as good means of attracting traffic below bridge on the Thames, and of improving the communication between Liverpool and Birkenhead, and other places similarly situated. The principle can easily be applied to existing piers, and may be readily arranged in such a manner as to suit any spot where it may be required. It can be placed either at right angles to the shore line, parallel to it, or at any desirable angle. It may be constructed with any number of supports, with one or any number of pontoons, with one or any number of girders, and of any strength which may be considered necessary.

The advantages claimed for the invention are as follows:—(1) A great saving in the cost of transporting goods across all such rivers as the Thames (below bridge), the Mersey, etc. (2) A very strongly-marked relief to the overwhelming traffic of the city of London. (3) A most important saving in time and expense in conveying railway trains, direct and intact, from one side of a tidal river or arm of the sea to the other. (4) A great saving in the construction of both piers and approaches as compared with those at present in use.—*Iron.*

ORDNANCE AND NAVAL.

A NEW BREECH-LOADING GUN.—However great may be the difference of opinion among artillerists concerning the relative merits of breech and muzzle loading guns, it will, we think, be readily conceded that Sir William Armstrong and his partners deserve great credit for the perseverance with which they have attacked a very abstruse mechanical problem. It would seem that they have at last achieved a real success; in other words, the firm have recently turned out a breech-loading gun weighing forty tons, or thereabouts, which appears to be free from the prominent defects of all other breech-loading ordnance. The new gun is constructed on the coil principle and has a bore of 12 inches. The weight of the servile projectile is 700 lbs., propelled by charges of from 160 lbs. to 170 lbs. of pebble powder. The breech is closed by a screw in a way very similar to that adopted by French artillerists, and described and illustrated in our columns. The breech screw is cut away by channels slotted through the threads, and the worm in the breech of the gun is similarly treated. To close the breech the screw is entered in such a way that the threaded portion meets the slots in the gun, the threaded part of the gun in like manner taking the slots in the screw. A turn of one-sixth of a revolution interlocks the threads and makes the breech secure. The gas check consists of a steel saucer, resting on a slightly convex surface on the inner end of the screw. The edge of this saucer is forced against a small rebate in the gun, and by setting up the screw it is made to take a firm bearing against this rebate; when the screw is released the cup contracts by its elasticity, and comes out with the screw with perfect facility. The screw is supported on a hinge, and can be swung back out of the way when the breech is opened. It is easily worked

by one man. The projectile is at once made gas-tight, and given its motion of rotation by a copper ring hooped round the base of the shot, which ring is upset into the grooves when the powder is ignited. Several guns have already been made, but of small size, on this principle at Elswick, but the forty ton gun is the first of its size. It has just been tested at the Elswick proof ground, some forty miles north of Newcastle, in the presence of all the leading artillerists of the day. The gun was fired several times with charges varying from 160 lbs. to 180 lbs., the initial velocity being very high, reaching as much as 1,615 feet per second, representing an energy of over 12,000 foot-tons. The experiments were perfectly successful in every respect. Of course only prolonged firing can demonstrate the powers of endurance of the new gun, but there is every reason to believe that these will prove sufficient for every necessary purpose.

FOREIGN NAVIES.—The Navy Department of Turkey is one of the eight ministerial departments of the Divan. The two largest ironclads are the sister ships Mésondivé and Mendonhijé, launched in 1874. They are 9,000 tons displacement, have a main deck battery of twelve 18-ton guns, projectile 400 lbs. The forward and after ports of the casemate are cut at an angle, so as to answer for bow and stern chases. The casemate is of 12-inch iron, and the hull is protected by 12-inch belt, the deck forward and abaft the casemate being shell proof. The spur is of unusual strength and below water. Two 6-ton guns on forecastle, fire directly ahead, one gun abaft of same calibre, fires directly astern. On spar deck are six 20-pounders, probably for saluting—total 21 guns. The Azizieh, 900, 16 guns; the Orkanieh, 900, 16 guns; the Mahmoudieh, 900, 16 guns; the Osmadieh, 900, 16 guns; and the Athor-Terfik, 750, 8 guns. There are also five ironclad corvettes carrying four or five guns each, and the two monitors, Hejzie-Rahman and the Loufan Djelil, carrying two 150-pounders each, and two light guns. The twin-screw ironclads Aoni Illah (Help of God) and Muin Taffer (Aid to Victory) are sister ships of 1,400 ton armor, five and a half inch. Four 12-ton rifle Armstrong guns, 250-pounders, are mounted in a central battery, so arranged as to admit being fired ahead or astern. These two vessels are said to possess very high rate of speed.

Following the above paragraph the appended information will be read with considerable interest. A correspondent of the Times, writing on the condition of the Russian ironclads, says, with regard to the Peter the Great: "She is so weak that if driven through the water at a speed greater than eight knots she shakes to such a degree as to leak in an alarming manner. Although the extent of her longest voyage is the distance between Cronstadt and Revel, her boilers are already under repair, and a commission which was lately assembled to examine her has expressed an opinion that all the large steam pipes should be renewed. As to her present capabilities for either offensive or defensive purposes, it is sufficient to say that when her heavy guns are fired rivet heads fly about unpleasantly. I think I have said enough about this ironclad, the pride of the Russian Navy." He goes on to say: "Now a few words with regard to the Popofkas. It is literally dangerous to fire the guns on board them. During a recent gun practice on board one of them, near Otchakof, the following results were obtained: At the first round almost every man on board was knocked down; the whole of the super-structure on deck was blown away, and the deck itself, which is iron-plated, was considerably bulged in a downward direction. I here beg to remark that the accuracy of the statements contained in this letter can be easily tested by any one who will take the trouble to make the necessary inquiries in this capital (St. Petersburg)." Mr. E. J. Reed in commenting upon the statements of this correspondent, says:—"He gives as a consequence of ordinary gun practice in the circular ship near Otchakoff, what I do not hesitate to pronounce a very exaggerated statement, and he refers me to St. Petersburg for proofs. But being already in possession of the facts, I am able to speak in the above terms of his statement and to add that the blowing down of some bulkheads in the superstructure and the depression of part of the deck were consequences of a most unusual accident, the gun having been fired by oversight over an open hatchway which ought to have been closed during gun practice. The natural consequences ensued."

The Dreadnought, double turret-ship, went out of Portsmouth Harbor recently, for a preliminary trial under way of her machinery, which was under the sole control of Mr. Robert Humphrys, of the contracting firm. Everything passed off with the greatest success. The blast was not once used, nor was it considered necessary to remove the ashes to increase the draught. The engines easily realized sixty-nine revolutions a minute, while the power developed was considerably over the contract power of 8,000 horses. The speed obtained was about fifteen knots an hour. The ship was so sensitive that she readily obeyed the slightest touch of the helm. On Friday, 5th Jan., the six hours' trial was made, in very boisterous weather. The wind blew strongly from the south-west, and at one time such banks of fog and rain clouds rose from the Isle of Wight and spread themselves over the sea that it appeared as if the trial would have to be postponed. There was some difficulty experienced in catting the anchor, in consequence of the carrying away of a ground chain; but the anchor was placed on the bill-board by about ten o'clock, and the ship got under way from Spithead at once. The wind had increased somewhat in the meantime until at its highest it blew with the force of from six to seven. The Island, however, acted as a capital breakwater, and the runs were confined within the Nab. In running up and down the measured mile course the ship was on several occasions timed, when it was found that a mean speed of 14½ miles had been obtained. This was highly satisfactory, but even better results will be obtained when the mile trial is made. The

draught of the ship on Friday, 6th Jan., was only twenty-one feet eleven inches forward and twenty-four feet six inches aft, whereas her estimated load draught when ready for sea is twenty-six feet eight inches forward, and twenty-seven feet two inches aft, or rather more than the immersion of the Thunderer. As the trial was not only for the purpose of enabling the contractors to obtain the covenanted horse-power out of the engines, but also for the purpose of ascertaining the consumption of coal in proportion to power, the boilers were easily fired in order to keep down steam. This was rendered all the more necessary in consequence of the boisterous character of the weather, for no sooner did the ship give a lurch or indulge in a roll, which she did whenever she went about, than the spring safety-valves lifted, and the steam escaped with a rush. With smooth water, consequently, it is very probable that even better data would have been obtained.

BOOK NOTICES.

THE CHEMIST'S MANUAL. By HENRY A. MOTT, Jr., E.M., Ph.D. New York: D. Van Nostrand. Price $6.00.

This work is an accumulation of practical notes designed to afford to the chemist a ready reference book of sufficient scope to serve any ordinary needs. To this end the author has skillfully arranged his work into the several departments of Qualitative Analysis; Mineralogy; Quantitative Analysis; Assaying; and in addition a Miscellaneous Department, which contains all tables in common use by the chemist. The test of practical utility seems to have been the only one applied by the author in selecting materials for the work.

A brief preface by Prof. Chandler, thus endorses the book:

"This carefully prepared manual of Dr. Mott will prove especially valuable, as containing a judicious selection of the most important methods, most of which have been tested by laboratory experience, and found to give satisfactory results. These are presented in concise form with reference to original authors. The numerous tables of constants will also be found of great value.

"This work will possess a special value for the student and laboratory worker, and will serve as a useful reference book for the general scientific reader."

ELECTRICITY AND THE ELECTRIC TELEGRAPH. By GEO. B. PRESCOTT. New York: D. Appleton & Co. For sale by Van Nostrand. Price $5.00.

The author aims in this work to present an accurate and comprehensive summary of Electricity and Telegraphy.

No pains have been spared in the way of illustration to render the author's descriptions perfectly clear.

In addition to explanations of all systems of Telegraphy worth describing, a complete compend of the laws of practical Electrical Science are carefully and logically presented.

The author's previous work was widely read, and served for a long time as the best known reference book on telegraphic systems. It enjoys yet a reputation which technical books rarely enjoy, of being readable. The present work is designed to take its place; it is fuller of scientific description and essay, but we doubt not will be as widely appreciated.

A TEXT-BOOK OF MINERALOGY. By EDWARD SALISBURY DANA. New York: John Wiley & Son. 1877. Price $5.00.

The preparation of a text-book on mineralogy was begun several years since by Prof. J. D. Dana, but the state of his health compelled him to relinquish it. The author of the work before us assumed the editorship, and completed the work substantially on the original plan.

The work is intended to meet the requirements of class instruction. It is presented in three divisions or Parts. Part 1 is devoted to Physical Mineralogy, and is subdivided into Crystallography and Physical characters of Minerals. Part 2 presents Chemical Mineralogy, and Part 3 Descriptive Mineralogy. This latter portion is only an abridgement of Dana's System of Mineralogy, which is known wherever there is any interest in the subject.

The chemical formulas are of the new system. The section on Crystallography is after the method of Naumann, and is an elaborate presentation of the subject. A colored plate of polarization phenomena embelishes this very complete book.

THE ELEMENTS OF MACHINE DESIGN. By W. CAWTHORN UNWIN. London: Longmans, Green & Co. 1877. For sale by D. Van Nostrand. Price $1.50.

This is an introduction to the principles which determine the arrangement and proportions of the parts of machines, and a collection of rules for machine design.

So far as the principles contained in this book have been expounded before, it has been as a part of Applied Mechanics.

The topics treated by chapters are in order as follows: Materials of Construction; Straining Actions; Resistance of Structures to Straining Actions; Riveted Joints; Fastenings; Pipes and Cylinders; Journals, Pivots, etc.; Bearings for Rotating Pieces; Toothed Gearing; Belting; Rope Gearing; Linkwork; Pistons, Valves, and Cocks

The book is the latest addition to the series of text-books of science.

ELEMENTS OF CHEMICAL PHYSICS. By JOSIAH P. COOKE, Jr. Third edition. Boston: John Allyn. For sale by D. Van Nostrand. Price $5.00.

This is doubtless the best work in Chemical Physics in the English Language. It is well-known to chemists everywhere, and the pages of former editions have afforded the best materials for the construction of our modern text-books for students.

There is no claim of any addition or improvement upon the former edition, and so far as we know, none was called for to meet the demands of scientists.

A reduction of this work to smaller dimensions formed the well-known Chemical Philosophy by the same author.

GENERAL REPORT OF THE MINISTER OF PUBLIC WORKS OF CANADA, for the Fiscal Year ending June 30th, 1876. Ottawa: Maclean, Roger & Co.

This is of the public document variety of literature, and is not widely different from the others of its kind.

The information contained is rather of the geographical and commercial order of statistics, than of the engineering kind. A very useful map is folded in the report.

THE COMBINED NOTE-BOOK AND LECTURE NOTES, FOR THE USE OF CHEMICAL STUDENTS. By THOMAS ELTOFT, F.C.S. London: Simpkin, Marshall and Co. 1876.

There are so many helps for chemical students that we are almost at a loss to imagine why there should be another. The present work does not afford us any great amount of assistance in making a conjecture. It reminds us strongly of Frankland's "Lecture Notes," interleaved with blank pages, and really contains little which is not already in that useful text-book. However, that which suits one man does not always suit another, so doubtless some will prefer the work before us; and for the benefit of these a little more care will be well bestowed upon the next edition, both as to clearness and consistency of expression, as well as to printing.

DYNAMICS OR THEORETICAL MECHANICS. By J. T. BOTTOMLEY, M.A., &c. London: Collins, Sons & Co.

This handy work is one of the Messrs. Collins' well-known elementary science series, and although not very voluminous, contains a mass of information in a neat and comprehensive form. In a short introduction the learned author (who, by the way, has lectured and demonstrated in natural philosophy in the Glasgow University) explains the meaning of the word dynamics, which of late had been incorrectly included in the term mechanics. "Dynamics," he says, "or the science which treats of force and its effects in general, is subdivided into two branches, to which the names Statics and Kinetics are given. Statics is the branch which treats of the balancing of forces. . . . Kinetics is the part of dynamics which treats of forces acting against inertia of matter." On this joint basis he continues his work through eight well-written and highly instructive chapters, and in conclusion gives a number of exercises and some tables of comparison of British and metrical measures. No student in this branch should be without this work.

SCIENCE LECTURES AT SOUTH KENSINGTON: OUTLINES OF FIELD GEOLOGY. By PROFESSOR GEIKIE, LL.D., F.R.S.; THE ABSORPTION OF LIGHT AND THE COLORS OF NATURAL BODIES. By PROFESSOR STOKES, F.R.S. London. 1876. For sale by D. Van Nostrand. Price 20 cts.

Whatever opinions may be held as to the expediency of continuing the lecture department of the late Loan Exhibition at South Kensington, there can be but one as to the excellence of many of the discourses there delivered. The two lectures by Professor Geikie form a case in point. They give, with admirable clearness and with commendable brevity, a complete outline of their subject, calculated to serve as a valuable aid either to the student who wishes to become a practical geologist or to the teacher who desires to show his pupils how they may learn in the open air. Professor Stokes' two lectures on absorption and fluorescence are precisely what might have been expected from a master of subjects which he has made his own, and they can be recommended as excellent, if brief, monographs upon certain points of the science of light.

MISCELLANEOUS.

CLEVELAND IRONMASTERS' RETURN.—These monthly returns were issued lately. They show 108 furnaces blowing, as against 112 in Sept., 1875; 55 out, as against 47 in September, 1875: and four as being the increase in the number erected since September.

M. LECLANCHE has just constructed a new galvanic battery, which the *Annales Industrielles* thinks likely to render great service both in manufactures and in scientific research. The original oxide of manganese battery by the same inventor consists of a porous jar filled with pyrolusite, in which is contained the carbon forming the positive pole. This jar is immersed in a solution of sal-ammoniac in contact with zinc. M. Leclanché has, however, introduced several improvements into this battery. He has superseded the porous jar by conglomerating the oxide of manganese, mixed in nearly equal parts with carbon, but with the addition of a small quantity—five per cent.—of resin for the purpose of giving consistency to the mass. These three substances, properly pulverized and intimately mixed, are conglomerated under a considerable pressure, and at a temperature of about 100 deg. Cent.—212 deg. Fah.—into a solid cylinder, which serves at the same time as a porous diaphragm and a positive electrode. But here a difficulty occurred. Under the influence of the current, an almost insoluble oxychlorate of zinc was formed, which was deposited in a crystalline form in the pores of the electrode, and considerably diminished its conductibility, so that the internal resistance of the battery increased very rapidly, occasioning considerable inconvenience, especially when used for telegraphic purposes. M. Leclanché has now got rid of the difficulty by inserting in the center of the carbon and manganese electrode, while being moulded, a small cylinder of bisulphate of soda. This acid salt prevents the formation of the oxychlorate of zinc; and the battery preserves its regularity for more than a year without the necessity for renewing the water of the saline solution. It offers a much slighter resistance than other batteries, and gives out a considerable quantity of electricity. A single element of small size, presented by M. Du Moncel to the Academie, immediately caused a platinum wire connecting its two poles to become red hot.

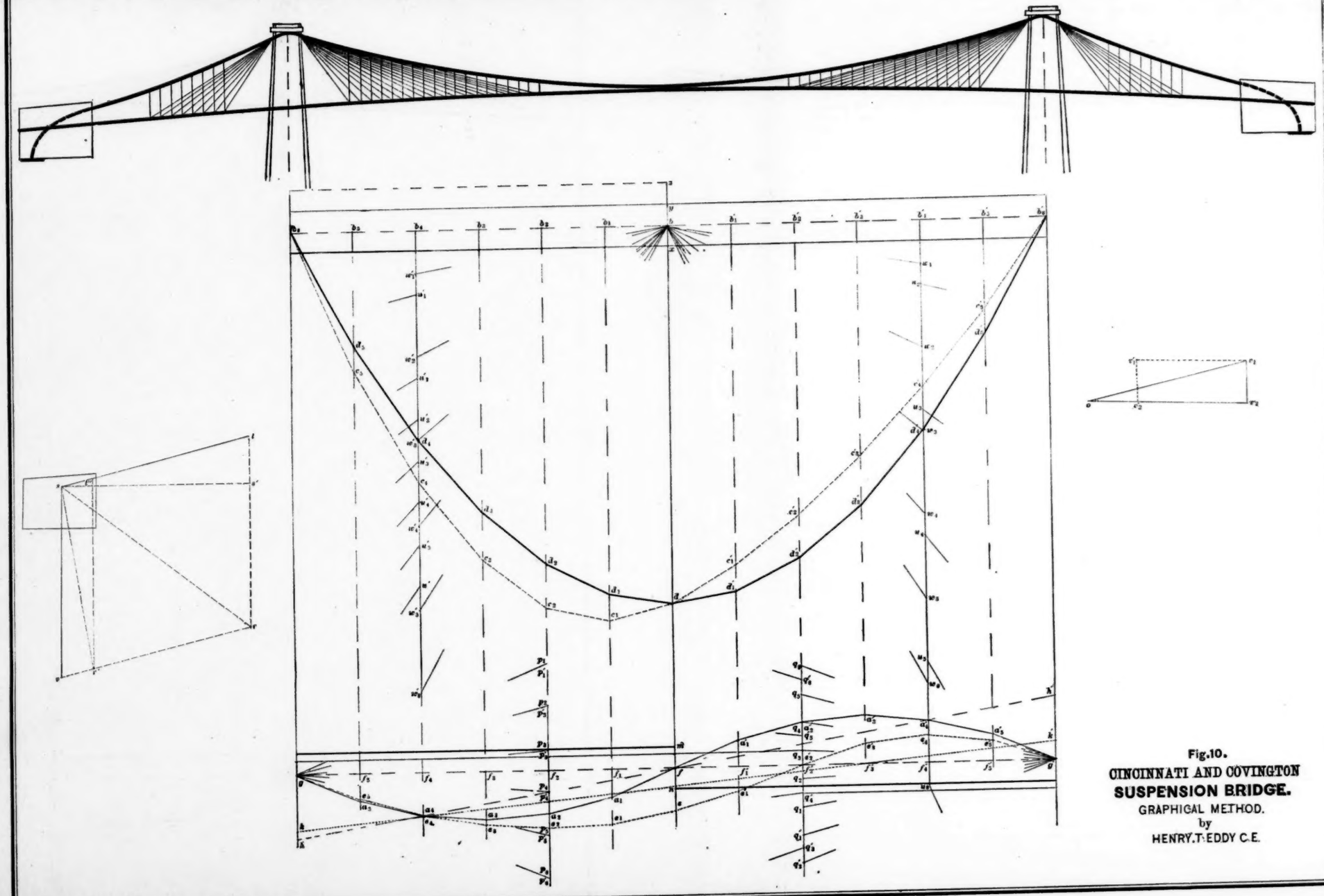

Fig.10.
CINCINNATI AND COVINGTON
SUSPENSION BRIDGE.
GRAPHICAL METHOD.
by
HENRY.T.EDDY C.E.